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THE PSYCHOLOGICAL REVIEW

CONSCIOUSNESS AND MOTOR RESPONSE[1]

BY HERBERT SIDNEY LANGFELD

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"Tous nos organes de perception sont à la fois sensoriels et moteurs," wrote Ribot thirty-seven years ago, and he remarked further, "sans éléments moteurs, la perception est impossible."[2] A somewhat radical statement at the time, and to one who had been brought up on traditional psychology with its dominant metaphysical and theistic concepts, it seemed almost as naïve as does Behaviorism to some scholars today. In an attempt to explain rather than merely describe mind, however, I soon found purely mentalistic concepts unsatisfactory, even though one understood them to be functional in character. Although we vigorously criticized the old soul and faculty psychology, we were, and still are, haunted by their ghosts. 'Attention,' for example, as a purely mental function (one could as well write 'faculty'), is honored with a separate chapter in most textbooks, and even in recent descriptions of experimental work we read that attention is directed now here, now there, and changes the phenomena by the shift. As F. Wulf[3] has stated, attention has had to be the psychological maid-of-all-work, and one could add, a poor maid at that. Further, we find that 'thoughts direct' and 'ideas join forces' and 'guide action.' Such mental descriptions are indeed verbiage. They bring us merely to the beginning of our problem and have as much reality as have the names of diseases in medicine.

[1] Read at the VIII International Congress of Psychology, Groningen, 1926.

[2] Ribot, Th., 'Psychologie de l'attention,' Paris, 1889, p. 74.

[3] Wulf, F., 'Beiträge zur Psychologie der Gestalt,' VI, *Psychol. Forsch.*, 1922, 1, p. 368.

On the other hand, experimental evidence has seemed to me to indicate with increasing clearness the rôle which the motor response plays in an explanation of mind. Today I can accept Ribot's statement and merely desire to broaden it by the substitution of consciousness for perception. Consciousness, as E. B. Holt[4] has affirmed, is identical with specific response of the organism. Wherever we have stimulus, afferent excitation, and integrated response, there we have consciousness. Such a doctrine is behavioristic, but not one to which J. B. Watson would entirely subscribe. Consciousness is a phenomenon; not something over and above physiological processes, or parallel to them, but identical with them. Nor is consciousness to be identified merely with the afferent current and stimulation of the central nervous system, leaving motor response as an effect produced by the conscious process. Such a concept would not be far removed from mentalism, for consciousness as thus defined would remain an agent with its principal abode in the brain, and if it were not for recent physiological findings, many of those who hold this theory, would still believe that there were separate and isolated compartments for the various functions of seeing, hearing, etc., and the various perceptual processes, so slow are we to break from the conceptions of the neophrenologists.

Instead of identifying consciousness with the afferent impulse, I believe that wherever there is the phenomenon we call 'consciousness,' there we shall find both afferent impulses and motor impulses to the striped muscles. These latter impulses may be too weak to produce overt movements, but the point is that the efferent impulse must take place. I cannot agree, however, with the otherwise excellent exposition of Dashiell[5] that the response may be also to the smooth muscles of the glands. Such responses only become a factor of consciousness in so far as they are in turn stimuli for the specific response of the striped musculature. On the other hand, spinal re-

[4] 'Response and Cognition.' Appendix to 'The Freudian Wish,' New York, 1925. The concept is further elaborated by Holt in a forthcoming book entitled 'Animal Drive, Instinct, and the Learning Process.'

[5] Dashiell, J. F., 'A Physiological-Behavioristic Description of Thinking,' Psychol. Rev., 1925, 32, pp. 54–73.

flexes affect and alter the integration of the higher centers. As Herrick has stated, " . . . No simple sensory impulse can, under ordinary circumstances, reach the cerebral cortex without first being influenced by subcortical association centers, within which complex reflex combinations may be effected and various automatisms set off in accordance with their preformed structure."[6] And further on, the same author has pointed out that "the efferent cortical discharge may become a factor in the local reflex stimulation of a motor spinal neurone."[7] In short, it is not justifiable to single out a single segment of the neuro-muscular mechanism involved in a specific response as being alone conscious. The entire system is integrally related and must be considered as a whole. The beginning of this inter-relationship is observed genetically in the work of Kappers, Child, Harrison, and Detwiler, where it is demonstrated that stimulation determines the growth and connections of neurones, and we have also the work of Pawlow on the conditioned reflex, which proves that the final path of the conditioned afferent impulse is determined by the condition of the response mechanism at the time.

Consciousness is often limited to introspective awareness. The definition of consciousness as specific response, however, evidently includes mental states, of which we are not introspectively aware. That there are different mental states—mental being used in the broad sense, to cover the so-called unconscious,—we must admit, and these differences must be explained in terms of differences of complexity, or differences of organization of the nervous pattern, but as Lashley has stated, "In modern psychology, with its hierarchies of the subconscious, the dividing line between conscious and unconscious has ceased to be of importance, relative to the dynamic features pervading both."[8]

In the above description of consciousness my aim has not been to present a concept of consciousness, but to show the

[6] Herrick, C. J., 'Some Reflections on the Origin and Significance of the Cerebral Cortex,' *J. Animal Behav.*, 1913, 3, pp. 228–229.

[7] *Idem*, p. 235.

[8] Lashley, K. L., 'The Behavioristic Interpretation of Consciousness,' II., Psychol. Rev., 1923, 30, p. 341.

part I believe the efferent impulse and motor response play in such a concept, in order to avoid any misunderstanding of what follows. My main purpose is to present a few facts which seem to me to strengthen the theory that the motor response is an integral part of consciousness.

Recently Dr. Carmichael and I have performed a series of simple experiments upon the well-known Poggendorff illusion of the two vertical parallel lines crossed by a diagonal line. The part of the diagonal line to the right of the parallel lines seems displaced relative to the part to the left. We presented two different versions of the illusion. In the first setting we showed merely the plain lines. In the second one, we made the parallel lines represent a column and the diagonal line a cord which passes over a pulley at each end, and seems to be pulled taut by a man at each end.[9] In the second setting the illusion is decreased. Our purpose was to investigate the illusion under controlled conditions, and to discover the amount, if any, of this decrease. Our procedure was to place the right hand diagonal line at a point where it was evident that it was not a continuation of the part to the left and then gradually to raise it until it seemed to be a continuation. This method was used both on the simple lines and on the lines with added meaning, with an interval of several days between the two series. There were 12 subjects, and each subject made twenty judgments under each condition. We found that the average decrease in the illusion when the cord seemed to be pulled was twenty-six per cent with an M.V. of 7.2. These results seem to me to be caused by increased empathy, but not in the Lippsian sense of a purely mental phenomenon. According to the motor theory there is a changed response, and altered condition of the muscle tonus. The Configurationists, I am aware, have a different explanation, but I wonder if the difference is not due in part, to a misunderstanding. I agree that the stimulus acts as a whole There is no analysis of the parts during perception, nor is there a mere association of ideas. Also, the configuration

[9] Filehne, W., 'Die geometrisch-optischen Täuschungen als Nachwirkungen der im körperlichen sehen erworbenen Erfahrung,' *Zsch. f. Psychol.*, 1898, 17, p. 35.

must be accounted for in physiological terms, and here too, we must speak of a configuration or system of interdependent parts. It will probably be objected, however, that in such perceptions there is no evidence of kinæsthesis, and that a visual experience is totally different from muscular movement. I would entirely agree with this view also. For consciousness there is purely and simply a visual experience of a displaced line. There is, however, here a specific response—a definite reaction of the organism to the particular situation, and such reaction involves both afferent and efferent paths. In physiological terms, the configuration is a unity of both sensory and motor stimulation, and on this point I doubt whether the Configurationists will agree, for their explanations, so far as I am aware, have always been in terms of afferent impulses and central integration.

To return to the question of empathic response. When I speak of empathy in æsthetic experience, (which is only one form of empathy, as all perceptions of external objects according to this theory are empathic), I do not mean that the experience is one of kinæsthesis. Such conclusion can only come from a false laboratory setting or incorrect interpretation of results. A perception of kinæsthesis is a perception of kinæsthesis localized within one's organism, and not the perception of a wavy line or an upright column any more than a perception of the movements in the vocal organs is the thought of a polar bear. The perception of one's movements as one stands before a statue is not a perception of the grace of the statue. The description of the perception of a work of art in motor terms leaves many persons cold, for the experience one usually gets, if not set to introspect, is one of beauty of line and contour, of weight, movement, etc. In one instance, one is set to respond to movement, and in the other, to the statue. The two attitudes are different and therefore the nervous integration is different, yet in both instances the complete nervous arc is involved. Our motor impulses underlie the æsthetic experience just as much as they underlie the kinæsthetic experience. What has been said of the æsthetic experience also can be said of the illusion just described. The

illusion is the content of consciousness, and the motor impulse can only be inferred indirectly.[10]

The place of the efferent impulse can be beautifully illustrated in experiments on the apparent size of the moon, which were performed at the Berlin laboratory.[11] It was shown that the size of the moon varied in proportion to the amount of effort necessary to overcome the divergence of the eyes when the head was raised. There is also an indication that the innervation of the neck muscles was a factor. (The recent work of Magnus,[12] in which he shows that the change in muscle tonus of the neck muscles changes the tonus of other muscle groups, lends weight to this last assumption.) The perception, however, is a visual one of the size of the moon, and not one of eye muscle strain or twisted neck. In fact, introspection never revealed the part that muscles played in the illusions, and every other form of explanation, both physical and physiological, have therefore been offered to solve the problem.

Eye movements have long been recognized as an explanation of the visual local signs by the geneticists, but usually the kinæsthetic sensations from such movements have been supposed to be transformed in some mysterious manner resembling alchemy into visual space experience. In the phenomenon of localization of touch on the skin, movement seems to play an even more obvious part than in visual space. The localization, insofar as introspection is concerned, seems to be obtained by a certain perceptual *quale* or local sign quality, but the fact that localization is best in those parts of the body which take part most frequently in the movement experience of the individual, points definitely to motor impulses. Additional proof of the rôle of movement in localization is found in a report by W. O. Krohn,[13] who states that a man who had an arm broken

[10] For the distinction between consciousness of movement and the experience of æsthetic contemplation, see the writer's 'Æsthetic Attitude,' New York, 1920, pp. 117–118.

[11] Schur, E., 'Mondtäuschung und Sehgrössenkonstanz,' *Psychol. Forsch.*, 1925, 7, pp. 44–80.

[12] Magnus, R., 'Körperstellung,' Berlin, 1924.

[13] Krohn, W. O., 'Sensation-Areas and Movement,' Psychol. Rev., 1894, 1, pp. 280–281.

and in consequence had held it immobile for three months, had a two to four times larger two-point tactual space limen on the unmoved member than under normal conditions. Further, the size of a cavity in a tooth at first seems larger to the tongue than to the finger. Is it not suggestive in this case also that the innervation of the tongue muscles is different from that of the fingers? Few persons, however, will be aware of movement; the object of consciousness is the size of the cavity.

Cannot also the phenomenon of dynamogeny be better explained by motor response than by an enhancement of afferent impulses? Take, for example, an experiment in which the observer perceives as many letters as possible tachistoscopically while a bell is rung at the same time close to his ear. It was found that the observer whose visual perception was most increased by the auditory stimulus was not disturbed by the bell, but was indeed entirely unconscious of it. One would say, according to the motor response theory, that the auditory stimulus found an outlet in the efferent paths already open to the visual stimulus. The two stimuli had a common motor path and the motor response, which was one primarily to the visual stimuli, was thus enhanced, the result being an increase in the visual experience.

Attention in terms of motor adaptation is too well known to need to be more than mentioned here; and reasoning and meaning are problems of such complexity that it is almost useless to attempt to treat them in the short space at my disposal. I should like, however, to call attention to certain valuable suggestions in recent literature. It must be admitted that the authors have not explicitly advocated a motor response theory of consciousness, but what they have written fits well into such a program. Rignano bases his theory of reasoning upon the Denk Experiment as described by Ernst Mach.[14] Rignano writes "It (reasoning) would seem to be nothing else . . . than a *series of operations or experiments simply thought of*, that is to say, operations or

[14] Mach, E., 'Erkenntnis und Irrtum,' Leipzig, 1905, especially the chapter entitled 'Uber Gedankenexperimente.'

experiments that we imagine performed on one or several objects in which we are particularly interested, and that we do not perform actually. . . ." [15] In this short sentence it seems to me Rignano has given us the key to a response interpretation of thought, if we are permitted to interpret the last phrase 'do not perform actually' as meaning that the response is nevertheless present in the form of incipient motor impulses. This interpretation seems to be further justified by Rignano's remark that reasoning puts "the individual in the very same state of mental 'awareness' in which he would ultimately have found himself, if these operations or experiments had been performed not merely in imagination, but actually—a state of awareness in accordance with which he determines his own behavior." [16] There is no doubt too, that the response theory underlies Dashiell's remark, "If, now, we recognize that the process of thinking differs from any other trial-and-error activity mainly in degree of explicitness, are we not presented with another physiological conception of thinking, an alternative to the intra-cerebral conception, the poverty of which I tried to show . . ."? [17]

Malinowski in a brilliant paper illustrates by examples from primitive language and children's speech that a word's meaning is the action for which it stands. Words, in short, are conditioned responses to action, and only if we know this action, do we understand their meaning. As he states it, "In all the child's experience, words *mean*, insofar as they act and not insofar as they make the child understand or apperceive," [18] and again, "A word *means* to a native the proper use of the thing for which it stands. . . ." [19]

Ogden and Richards have based their theory of meaning upon the act of reference. I hesitate, however, to read too

[15] Rignano, E., 'The Psychology of Reasoning,' London and New York, 1923, p. 81.

[16] *Idem*, pp. 97–98.

[17] Dashiell, J. F., 'Is the Cerebrum the Seat of Thinking?' Psychol. Rev., 1926, 33, p. 18.

[18] Malinowski, B., 'The Problem of Meaning in Primitive Languages,' Sup. 1 of the 'Meaning of Meaning' by C. K. Ogden and I. A. Richards, London and New York, 1923, p. 487.

[19] *Idem*, p. 488.

much into their statements, as I am not sure what they mean by 'mental processes.' Still, it seems to me that in their definition of an engram they come very close to a response theory, for they write, "An engram is the residual trace of an adaptation made by the organism to a stimulus. The mental process due to the calling up of an engram is a similar adaptation: so far as it is cognitive, what it is adapted to is its referent, and is what the sign which excited it stands for or signifies." [20]

I have attempted in these few minutes at my disposal, to offer a possible explanation of certain psychological problems in terms of organic response. I realize, however, that there are many difficulties still to be overcome. Some persons will probably feel that the gaps which exist in the system can not be bridged, and that therefore the entire theory should be rejected. It seems to me, however, that the structure is sufficiently firm to stand, and that the response viewpoint suggests laboratory problems whose solution should be of inestimable value to psychology.

I also realize that psychologists use physiological terms much too glibly and tend to fall into the same error of verbiage as do the mentalists. Inhibition, organism as a whole, higher integration, etc., are often used by the psychologist more daringly than by the physiologist. You will remember with what hesitation Adrian [21] spoke at the last international meeting on the subject of nervous and mental energy. With the advance in physiology and biology, however, I feel convinced that we psychologists will see our problems with increasing clearness.

[20] *Idem*, p. 140.

[21] Adrian, E. D., 'The Conception of Nervous and Mental Energy,' VII. Internat. Congr. of Psychol., Cambridge, 1924, p. 158.

THE SIGNIFICANCE OF QUALITATIVE DIFFERENCES FOR PSYCHOLOGY [1]

BY J. B. MINER

The University of Kentucky

If systems of psychology are a symptom of maturity, the science in this country seems recently to have reached an adult level. Let us hope it is not senility. We may even be at the cradle of a new generation. A list of living American psychologists who have set forth more or less systematic outlines of the science in textbooks or treatises would include Adams, Angell, Bentley, Breese, Betts, Calkins, Carr, Dunlap, Gates, Gault and Howard, Griffith, Hunter, Judd, Kantor, McDougall, Major, T. V. Moore, Meyer, Ogden, Pillsbury, Read, Seashore, Smith and Guthrie, Stratton, Thorndike, Titchener, Troland, Warren, Watson, Weiss, Witmer, Woodworth, and Yerkes. Twenty-four of these thirty-three books have been published as new or revised editions in the last five years. They represent the psychological A, B, C's of this country. We have the announcement from Clark University that it will publish 'Psychologies of 1925.' This will include six varieties of systems. The royal road to distinction in psychology in the United States seems no longer to lie along the runway of a textbook. Nobody could charge that birth control had endangered psychological productivity.

The parents of these systems, Alpha to Omega, are naturally jealous of their offspring, and the kids themselves are likely to be scrappers. In the belief that neither dogmatic assertion nor invectives which display repressed complexes will permanently avail much, we may turn to one phase of the controversies which has been sensed by a number of the combatants and by-standers. There is a feeling that the delightful impulse to simplification is being over-indulged by

[1] Address of the president of the Southern Society for Philosophy and Psychology at Lexington, Kentucky, April 2, 1926.

some of the systematists. Kantor [2] seems to represent the extreme tentacle of sensitiveness to this tendency. In protest against the incompleteness of "an artificially simplifying biology," he says: "As students of human psychology, we must be capable of studying complex conduct in addition to the simplest behavior (sensory discrimination, motor reflexes, etc.) which can be described in biological terms. Is it not the attempt to biologize human phenomena which has resulted in handing over to novelists and psycho-pathologists the sole guardianship of the problems of human behavior and human personality?" Psychology must not be reduced to 'physiological functionings.' For him nothing short of the "reactional biographies of individuals throughout their various contacts with their surroundings" will be adequate for the understanding of human activities.

For McDougall [3] the "tendency to simplification is in fact the root of the mechanistic mosaic psychology, that which describes the mental processes as made up of static elements, 'units of feeling,' 'atoms of sensation,' 'neutral entities,' or what not." For him such psychology has been reduced to 'the lowest level of banality' by Bertrand Russell. He deplores the 'devil-may-care attitude' of Watson toward conscious experience and would also strike against the 'forlorn banner' of Holt's simplification. It is a question whether even 'purposive striving' would satisfy McDougall's desire to "lead the student up to the complexities of the human mind."

Watson, Meyer, and Weiss seem to lead in a willingness to abandon to the dogs the introspective observation of conscious responses. Many are glad to sound praises to their high scientific standard when they insist that science must stick to observable facts and their relations, and when they lead against any return of psychology to the abandoned fortress in which names are reified or deified.

The question of an over-emphasis on simplification brings us straight to the consideration of the bearing of qualitative differences on psychology. Without attempting a systematic

[2] Kantor, J. R., 'Principles of Psychology,' pp. xvii, 30, 34.

[3] McDougall, W., 'Outline of Psychology,' pp. ix, 27, 11.

solution of the controversies in psychology, let us restate certain problems in the light of qualitative differences, in the hope that a restatement may assist in their solution.

The Field of Psychology

The aims of science may be briefly stated to be the description and explanation of certain facts for the purpose of predicting and controlling them. Descriptions primarily answer the question, 'What is this'? Explanations answer the question, 'In what common sequences does it occur'? The usual sequences are causal or genetic. In both description and explanation the scientist makes use of quantitative differences. He asks, 'How much of this or that is there'?; and 'How much is necessary in order that the sequence may proceed'?

To make a clear distinction between quantitative and qualitative differences let us define quantitative differences as those which can be stated in terms of amount or position. Two facts or relations are *qualitatively* different when one cannot be described completely in terms of a different amount or arrangement of the other. A new quality is discovered when there is something in the fact or in the relation more than in the mere summation of the qualities previously described.

We may pass over the question whether a chemical compound shows qualities not found in the atoms which compose it. We may even neglect the problem whether physiological activities include qualities not present in the physical activities. The important question in describing the field of psychology is whether the responses of an *individual* include qualities which cannot be adequately described from the point of view of another individual? Is there something omitted in the description of an individual which is restricted to his nervous, glandular, or even his total physiological responses?

Those who would limit psychology to the science of conditioned reflexes or to the description and explanation of the activities of 'the other one' would probably admit that conscious facts are of a qualitatively different order. They would take the position that such facts are negligible for the

purpose of controlling the behavior of individuals. This would place the burden of proof on those who would utilize conscious facts, to show that such facts (conscious sensations, affects, images, meaning, purpose, etc.) have sometimes been useful descriptions for controlling ourselves or others. The introspectionists, on the other hand, might contend that it is the business of psychology to describe differences as well as similarities. The psychologist, they might say, should accurately describe conscious facts regardless of whether he can at present explain them or utilize them for controlling behavior. Probably there is no introspectionist who does not believe that the uniformities discovered among conscious facts are useful for the control of behavior.

The problem of the field of psychology may then be restated in relation to qualitative distinctions. It is the question whether psychology shall limit itself to the science of objective responses, both as facts to be described and as means of controlling behavior. Shall it concentrate its energy on simpler and more fundamental activities, with the danger of neglecting those that are most directly related to understanding particular individuals, to controlling the rarer and more complex events? Or, shall it follow wider fields with the danger of dissipating its energy so that it will progress quite slowly upon those paths which affect the understanding of more elemental responses? Is not psychology broad enough to recognize the value of both types of interest?

The Cause and Effect Relationship

The recognition of qualitative differences is most significant when we consider different types of explanation in psychology. In particular, let us consider causal and genetic explanations. The concept of cause is notoriously slippery. We should be sure if we apply it to a psycho-physical sequence that it has the same meaning that it does in physics, or else we should use another term for such a series. As commonly used in physics the cause and effect relationship implies three things about a sequence: First, that it shall be a uniform sequence; second, that the cause and effect shall both be

statable in the same qualitative terms, that there shall be nothing in the effect not in the cause. Third, in the physical world, the change which takes place in cause and effect is statable in terms of change of motion. Change in motion may be stated as change of position, direction, or speed. The effect must show no new quality not found in the cause. It must, therefore, be statable in change of spatial relations only.

It is this physical concept of cause which is said so often not to apply to the relation between mind and body. We cannot conceive, it is said, an *idea* of a beefsteak causing two molecules in the brain to bump together. Conscious facts and physical facts are qualitatively different. Their differences are expressed in the humorous descriptions: "What is mind? No matter. What is matter? Never mind." The psychologist may be able to dodge the metaphysical problem of the relation of mind and body; but he can not escape the necessity of keeping his meaning clear. When he uses the term cause he should use it in the same sense that it is used in physics, or else he should recognize that the psychological sequence is of a different type, a qualitatively different sequence.

The behaviorist movement has made an important suggestion regarding the dilemma of the sequence of conscious and bodily facts. It has done so by the use of concepts in which the sequence from stimulus to response can be described without making qualitative distinctions. The concept of 'behavior' permits of a single qualitative continuum. We can speak intelligibly of behavior which may be either conscious or bodily activity, without raising questions of qualitative differences. 'Behavior' emphasizes the similar characteristics of both conscious and bodily activity. The description of a sequence in terms of either 'behavior' (or 'activity' when activity is thought of as mere change) thus affords a common qualitative term for the psycho-physical sequence. Montague contends that energy, potential and active, is also such a common term for the psycho-physical series. This is not true of the concept 'motion and rest' since we should not

speak of the spatial motion of conscious facts themselves, but only of the motion of the objects to which they refer.

Another concept which has come into prominence for expressing the continuity of the psycho-physical series is that of 'integration.' This is especially valuable when the organic nature of the series is considered. It is also essential in considering subconscious activities. All of these terms express the hunt for the factor of continuity in the body-mind series. They seek to use that part of the physical concept of cause which requires both uniformity of sequence and a single qualitative factor throughout the sequence.

An analogy may help to understand the nature of the behavior sequence in psychology. If we think of a temporal series of letters of the alphabet, some of the letters may be observable as facts which are, *in certain respects*, of qualitatively different orders, namely, small *d* and capital *D*, small *e* and capital *E*, and small *f* and capital *F*. Both small and capital letters, however, belong to the continuous alphabet-behavior sequence.

This analogy shows in what respects the behavior series is both adequate and inadequate as a description of psychological activities. It permits us to think of the psycho-physical series as a continuous uniform sequence of successive letters of the alphabet. These are qualitatively alike in being letters of the alphabet. On the other hand it shows us the inadequacy of the physical causal concept to explain the appearance of the qualitatively different capitals, *D*,*E*, and *F*, in addition to small letters. These conscious facts, represented by the capital letters, are qualitatively different from physical facts in several respects. For example, they do not show spatial characteristics and do show symbolic relationships. A conscious fact, as such, or the conscious aspect of a neutral fact, is not round or square although it refers to a circle or square much as x in algebra may refer to a mile in space, although it is not a mile long. So we may say a conscious fact does not have shape, size, position, motion, that is, spatial characteristics. Furthermore, conscious facts are often symbolic and physical facts are not symbolic; a stone

can not re-present to itself the quarry from which it came. All terms of the psychological sequence, however, do show the common quality of activity, or behavior. Behavior thus allows us to retain the concept of uniform sequence as a partial, but as an incomplete explanation, of the psychological series. The physical causal sequence is to be distinguished from the psychological causal sequence because under the physical concept, the appearance of the new qualities of conscious facts expressed by capital *D*, capital *E*, and capital *F* is not explained.

This analogy of the alphabet series shows how the *observation* of the continuous uniform alphabet sequence may be kept up regardless of whether all the elements in the series can be observed both as physical and as conscious facts. We may proceed from small letters to capitals, brain facts to conscious facts, back and forth at any point and still conceive of the series as a continuous alphabet. Under this analogy it is not necessary to suppose two complete alphabets, small letters and capitals running parallel or alternating. It makes unnecessary the monstrosity of assuming that the capital letters are a continuous series. It avoids the attempt to describe hypothetically unobservable facts such as conscious facts of which we are not conscious.

This analogy of the alphabet series has the advantage of shifting the emphasis away from the old problem: Are the capitals and small letters a parallel or alternating series? It suggests that for practical purposes, such as the control of the alphabet series, we have two methods of approach through the small letters *a*, *b*, *c*, or through the capital letters *A*, *B*, *C*. We may study the control of the series either through the small or through the capital letters or through both. To use an illustration the psychologist may be said to study the control of the emotion of fear either through bodily processes (glandular secretions, motor tensions, etc.) or he may study the control of emotion through conscious processes. In the latter approach, he endeavors to change the ideational situation. The fear of the small boy on falling into a river may be controlled or conditioned by habitually associating the

river experience with swimming, diving, and similar objective bodily responses. On the other hand, the approach to the control of the boy's fear on falling into the river, might be from the conscious side. He might be relieved from fright by the prompt remark of a by-stander. The by-stander could change the ideational situation by asking: "Did you see any mermaids down there"?

The effect of the general behaviorist movement has thus been to make the series more important than keeping the sequence all in physiological terms or all in conscious terms. It disregards, as relatively unimportant, the question whether some factors in the series are describable, that is, observable, in terms of only small or of only capital letters. Behaviorism asks only that the letter series be conceivably a continuum. The duplication or non-duplication of small and capital letters need not disturb the observable continuity of the alphabet. When this fear sequence is carefully thought through, however, it seems to distinguish the uniform sequence of physics, which is statable in the common terms of spatial motion, from the psycho-physical behavior sequence which is not always *fully* describable in spatial characteristics. If this analysis is correct, the psychologist should recognize that his uniform behavior sequence ought not to be called a physical causal sequence. The psychologist may, however, properly speak of it as a uniform behavior sequence, or a psychological causal sequence. All terms of the sequence are statable in a common qualitative term, in the term 'behavior.' Such a psychological casual sequence is to be distinguished from a physical casual sequence in that all the terms are not spatial, all changes are not necessarily observable as changes of motion and position.

The Genetic Relationship

The appearance of new qualitative characteristics in the psychological universe not found in the physical universe, points to the need for another concept of uniform sequence than those already described. It leads easily from the concept of the uniform sequence expressible in a common

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term to that of a genetic sequence. The genetic sequence is to be distinguished by the fact that later moments in the sequence show qualitatively different characteristics from those earlier in the sequence. This concept of genetic sequence pervades the whole science of biology. We sometimes do not realize that the biologist who observes a new species, a new structure, a new instinct, or a new habit, has discovered a qualitatively different fact. This new fact is not fully describable in terms of any observable characteristics which have gone before. The use of the term 'new' shows that this must be true. If there is a qualitative distinction between the 'new' and the 'old,' then there seems to be no logical alternative between a universe which is incapable of mutation except by new motions, and a universe which, from time to time, shows the genetic appearance of new structures or of qualitatively new responses. Such a qualitatively evolving universe implies that there occasionally occurs a new quality of fact or a new non-spatial relation which has not previously existed in observable form. Saying that these new qualities previously existed potentially or implicitly, of course, recognizes the fact that they were not before observable.

The usefulness of such genetic sequences for explanation and control is clear. They point to the place in the series where a new characteristic has commonly been found to appear. They enable us to predict its appearance on the ground of its common appearance in the past at certain stages. They suggest that stages in development can be best defined in terms of the predominance of qualitatively new facts or relations.

Let us take an illustration from psychology; again, perhaps, a debatable example, but one which helps us to think concretely. The concept of mental levels as expressing different stages in development has undoubtedly come to stay. We distinguish, on the basis of Binet testing, different mental levels or stages of mental development. For example, in the development of the *definition* process, the earliest level of this response in the child is described as 'definition in terms of use.' A child is asked, 'What is a chair?' He replies,

'A chair is to sit on.' This normally occurs at five years of age. At eight years of age, according to the Stanford Scale, the child normally reaches a higher level. Then the definition response includes a reference to objects of a wider class, a chair is now '*something* to sit on.'

Such a genetic sequence undoubtedly helps not only to predict what may be expected at different ages, but to control the training of children of the same age but of different intelligence levels. It gives us a new type of sequence which is commonly found and is of fundamental importance for understanding behavior. This type of change at a certain stage of growth or development we might well recognize and define as a genetic sequence. Whether or not all genetic sequences might ultimately be also statable as changes of motion need not be answered. It, of course, cannot be answered at present. If it should ultimately prove that all sequences are statable in terms of physical cause and effect, the differences between causal and genetic sequences during our present state of ignorance should be clearly recognized. Even if all facts were causally explained some of them might still have other types of explanation as well.

Purposive Striving and the Gestalt

The relation of qualitative distinctions to two concepts which stand for recent movements in psychology is of interest. Purposive striving and the *Gestalt* promise to demand our attention whether we would or no. Humphrey [4] has briefly stated the *Gestalt* criteria of von Ehrenfels and Köhler as follows: "First, that the structure is more than its parts—the example given in the original connection is that if the notes of a melody are each given to a different man, the resulting effect is not the melody, to obtain which the parts must be brought into 'functional relationship' with one another; second, the structure can be transposed (may occur with different specific tones); third, which is added by Köhler, is that the separation brings alteration of the parts." If my

[4] Humphrey, G., 'The Psychology of the Gestalt,' *J. Educ. Psychol.*, 1924, 15, 401–412. He also gives the references to the original studies.

understanding is correct, this third criterion means that a single tone of a melody, when separated, loses its melodic quality, is qualitatively different since its melodic function or characteristic is not there.

In some form this notion of configurations or patterns, possibly as patterns of integration, seems likely to aid in our classification of complex wholes. The conception of natural wholes which are qualitatively different from the summation of their parts is not new to psychology. The percept of an object is often regarded as something more than the sum of sensations and memories. It is a unit, an organic whole. The experience of an orange has been described as something besides sensations of taste, odor, color, contact, plus their memories and plus the motor attitudes during perception. Moreover, the elements of the percept, such as pure sensations, are not actually separable except for purposes of description. They are abstracted units. So too, the form of the percept might be partially at least transposed to other content.

The theory of the *Gestalt* has helped to define the qualities of a relationship which may be found not only in static objects of the laboratory and in common percepts but possibly in types of learning taken as wholes. Koffka points to *Gestalten* which are found in the learning processes of Köhler's chimpanzees as well as in other examples of mental development. If both spatial and temporal series may show the *Gestalt* relationship, including the functional interrelation of parts, this may mean much in clarifying our concept of organic relationships. It leads to the discovery of different temporal patterns of development among lower and higher organisms. The possibilities of enriching the understanding of genetic and organic relationships captivates the fancy. It is perhaps not too early to say that the concept of the *Gestalt* relation has both enlarged our conception of genetic explanations and simplified our explanation of more complex wholes. We explain by noting uniform temporal relationships; we may also progress by discovering new relationships. Is the configuration a particular form of the genetic sequence in which the new quality or object appears as a completed natural whole? If so, the classification of the *Gestalt* would be easier.

One advantage of the *Gestalt* concept over McDougall's 'purposive striving' is that it has not seemed to lead so easily to a revival of the old tendency to make names into causes. The *Gestalt* is not a subterranean thing which puts the pattern into the object. Its believers seem anxious to limit the concept to observable facts and their relations. McDougall also disclaims any reification of names. A purposive sequence may, he believes, be described in its relation to its end-result as a scientific observation of a sequence. By omitting any inference as to the thing directing the series a purposive sequence becomes perilously close to a *Gestalt*. The goal response completes a new event analogous to a new percept. Koffka uses the *Gestalt* relationship to describe a temporal series, such as an act of learning, in which the end may function with the beginning and other parts of the series to complete the whole. McDougall, however, seems not to be content with this. He says that the *Gestalt* is not sufficient to describe the characteristics of purposive action which he has in mind.

McDougall would make purposive striving the most fundamental category of psychology. He finds it in all animal behavior. He describes it among lower animals, however, not as an observable relation; but as an inference. It is an inference, he says, which we do not hesitate to make in the case of intended human behavior; but which might be more debatable in the case of lower organisms. The essential feature is that purposive striving is a reaction of the whole organism which one infers to be made for attaining a natural end and which is governed by an anticipation of this goal. Purposive behavior, he further states, may always be inferred in the behavior of lower animals when it exhibits certain characteristics. These characteristics are treated by McDougall as observable. They become the basis for the inference of purposive striving since they are found also in the purposeful actions of human beings. In purposeful human actions observable foresight of the end governs the succeeding events and moreover, he says, "effects precede and determine their causes." [5]

[5] 'Outline of Psychology,' p. 195.

Whatever we may think of McDougall's unusual use of the term 'cause,' or whether we accept or reject his concept of purposive striving, we can certainly be grateful for his admirable summary of the characteristics of organic behavior. The six characteristics of behavior which, he says, lead to this inference of purposive striving include: (1) "A certain spontaneity of movement." (2) "Persistence of the activity independently of the continuance of the impression which may have initiated it." (3) "Variation of direction of persistent movements." (4) The "coming to the end of the animal's movements as soon as they have brought about a particular kind of change in its situation." (5) "Preparation for the new situation toward the production of which the action contributes." (6) "Some degree of improvement in the effectiveness of behavior, when it is repeated by the animal under similar circumstances." McDougall believes that his concept of purposive striving describes a relation which helps psychology to explain behavior of lower and higher types by pointing to a common feature. He would "lead up to the complexities of the human mind by way of the simpler processes of the animal mind."[6] To do so, however, he infers as vaguely present in animal behavior what he finds clearly observable only in conscious human behavior.

From the point of view of qualitative differences the problem is whether purposive striving is ever an observable relationship which psychology has neglected and one which would simplify our grasp of other behavior if it were inferred to exist. Any psychologist who believes that it is legitimate scientific observation to describe conscious activities, might admit that a conscious process including foresight could not be adequately described in terms of conscious processes without foresight. Foresight would then differ from memory much as memory images differ from sensations by showing a new quality of reference not before observable. Koffka uses the illustration of the incomplete triangle as a forecast of the coming completion of a triangle which is the natural whole. He calls this 'closure.' Are not 'closure' and 'foresight' the

[6] *Op. cit.*, pp. 43-46, and p. xi.

same characteristic of a sequence which ends in the completion of a whole? The whole is taken to be qualitatively different from its parts, and probably also qualitatively different from its antecedents. The end-result could hardly have been predicted had it not been previously observed under similar conditions. May not purposive striving and the *Gestalt* be instances of the genetic sequence in which a new characteristic appears during the sequence or with the end-result? In other words, are not the *Gestalt* and purposive striving genetic sequences?

Under this view, the service to psychology of the *Gestalt* and of observable striving seems to be that they tend to bring out the distinction between a causal series and a genetic series. The objection to both would be in their introduction in any form, if they do, of an end-result as a physical cause. It is the old objection to regarding teleological purposes, later steps in a sequence, as preceding causes. We may insist on the difference between the physical causal or the psychological causal sequences and the genetic sequence in which a qualitatively new end-result appears. So far as conscious experience goes, neither the new solution of a problem toward which we strive nor a completed *Gestalt* seem to be describable in terms of the preceding partial events. Foresight and closure seem to be genetically late methods of control not observable at lower stages. Both seem to have reference to a different way in which a sequence may be governed than that which is described as physical causation in the inanimate world. Does this mean that we should add to our definition of the genetic sequence some concept other than cause as to the way in which the sequence is governed? If so, how is this notion of the control of a genetic series to be defined? The recognition of control by something like foresight has the merit of being the type of control man constantly strives to attain in his everyday living. The problem of another form of uniform temporal relationship than cause is one of logical analysis in which psychologists should be especially interested. Psychological facts most often suggest governing principles which at present are inadequately or inaccurately described in terms of

cause. This paper attempts to find or emphasize the problems, not to answer them. The recent discussions point to the question of how to improve the understanding of the genetic sequence by recognizing some other uniform relationship in it than cause.[7]

The question of the usefulness to science of the concept of purpose does not seem to have been settled historically. We still have with us the teleologists, vitalists, animists, etc. They still assert as does Driesch that the theory of association is dead in psychology, or is wholly inadequate for mental processes, as do McDougall and the Gestaltists. Is it not possible that these recent movements have properly emphasized a usefulness of ends and wholes for classifying facts and relations? Disregarding the danger of reification of ends or *Gestalten* as mysterious physical causes, is there not a real contribution to psychology in recognizing that any series running from 'a' to 'h' may be classified either in relation to 'a' or to 'h' or as a particular total series, or configuration. Theoretically there seems to be no reason why every series should be classified only in relation to its sources. In psychology in particular it is often easier to classify complex human behavior in relation to its end. The awkwardness of Thorndike's attempt to classify instincts in relation to specific stimuli and specific motor and organic responses is a case in point. In many respects McDougall's classification of instincts seems to be more useful and more hopeful. It affords better prediction and control when we think of the purpose of 'escape' than when we think of running, standing still, turning color, etc., with no group term which describes the common features of these responses in terms of their function.

Classification by ends has only to demonstrate its usefulness for explanation, prediction, and control, to justify its value to psychology. The principle of parsimony does not seem to require that a series which is classified in relation to its sources should not also be classified in relation to its end. Reading the series backward seems to be no simpler than

[7] Köhler believes that the causal re-establishment of physical equilibrium is adequate for a physiological picture of the *Gestalt*.

reading it forward. It is necessary only that the series shall include only observable facts, or inferences from such facts analogous to the inference of the electron and proton in physics or of Mendelian units in biology, which help to discover new combinations. If McDougall wishes to read purpose into the animal series by inference, however, it should be done on the ground that it is the same sort of inference used in assuming physical elements which are inferentially observed from their effects. Is it legitimate to infer a vague purpose of protection on the part of lower animals in order to point to the similar characteristics of animal and human conduct? Is it not better to point to the similarity of the animal and human behavior on the basis of the observable similarity of the series and the end-results? The inference of vague purpose may be gratuitous unless it helps to create or discover other end results.

Summary

In closing the consideration of the bearing of qualitative differences on current problems in psychology, we may summarize the suggestions brought together in this paper. A broad view of the place of psychology in science should recognize that psychology is especially favored in any effort to discover qualitative differences. The data with which it deals are more complex, more rare in nature and more likely to show new qualities of content and relationship. Psychology should, therefore, be especially keen to detect and to describe qualitative differences in facts and in their relations. The concept of physical causation might properly be limited to describing those features of a sequence which show no qualitative differences, in which the effect is identical with the cause in content and shows only differences in spatial position or movement. The expression 'psychological cause' might then cover sequences which include changes in a behavior sequence analogous to changes in the alphabetical sequence. In such a psychological sequence certain factors in the series might be accompanied by or described at present only in terms of conscious facts although the outcome be a change in motion.

'Genetic explanations' recognize the appearance of new qualities in the result. The genetic sequence is itself qualitatively different in that respect from the causal sequence. Both the *Gestalt* and purposive striving, when limited to the observable facts or relations and the useful inferences from them, may be scientifically helpful. They must, however, be freed from any tendency to reify names into mysterious physical causes. If thus freed from unnecessary adjuncts, they may enrich our concept of the genetic series by calling attention to new qualitative facts or relationships which are not found in non-conscious or non-psychological series. If so, they make a distinct contribution to the science.

The most fruitful movements in psychology for understanding rare and individual types of behavior, are those which recognize that psychological data may be expected to include differences which are in certain characteristics inexplicable, at least at present, in terms of the more general relationships found among the lower organic and inorganic activities. Social relationships, as suggested by Eldridge,[8] may include still other qualitative characteristics not included in those above described. The causal explanations of physics, under this interpretation, may be useful, albeit incomplete, explanations of certain orders of data. In other words, the discovery of continuity, of similarity, among physical, organic, and psychological data does not prevent the discovery of discontinuity, dissimilarity, differences. With the tendency to systematize now prevalent, we should be cautious about an overemphasis on noticing similarities.

Various points of view seem to be at least partially harmonized through the conception of a hierarchy of qualitatively different orders of integration. This at least is a synoptic conception which has been set forth by Eldridge, Hoernle[9] and others. The conception of a hierarchy of complexity which is not entirely described by quantitative differences in motion seems especially favorable for systematic thought in biology. It underlies all systems in which the genetic rela-

[8] Eldridge, S., 'The Organization of Life.'

[9] Hoernle, R. F., 'Matter, Life, Mind, and God.'

tionship is recognized as qualitatively different from the causal, and in which mutations and variations show new characteristics which are at least partially different from their causes.

Qualitative differences introduced by the psycho-physical series, offer new theoretical and practical problems for psychology. New qualitative relationships become increasingly important as we approach the less universal, more individual, types of behavior. Applied psychology and personnel work seem to emphasize the need for the description of qualitatively different types of relationship or at least the recognition of more examples of genetic relationship found in a conscious organism.

RECOGNITION AND RECALL

BY STEVENSON SMITH

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There is no definitive difference between recall and recognition. This is the thesis that I shall try to defend.

Both recall and recognition result from the situation that is present. Neither is spontaneous. Whether we describe them as consciousness or as movement, they are both the results of stimulation. Recognition comes about by the action upon our sense organs of the thing recognized. Recall follows stimuli that do not emanate from the object recalled. I recognize the sea by its color and pattern when I get into its vicinity. I recall it while inland if the mountain breeze is damp and cool. But suppose I have mistaken my road and am really on the shore though I do not know this. I have all the time been recognizing, not recalling, the sea. The case is classified by my position in an objective order.

I think I recognize the photograph of my friend, but I am mistaken. It is the photograph of his twin brother. I have merely recalled my friend's picture. The difference often rests upon such an historical event as which of two duplicate twins sat before the camera. It is frequently not a relation of present situation to response by which we define this difference. Did Hamlet recognize Yorick or did he recall him? Knowing the intelligence of janitors we may well doubt the authenticity of the skull.

It will be pointed out, however, that false recognition results from the similarity of the present situation to the situation we think we recognize. And this similarity is very significant. It is more important to know an egg when you see it than it is to know what egg it was that made that spot on your vest.

To recognize a class is in a sense to confuse the individuals that compose it. The insight of the baby, who generalizes by

calling all birds ducks, is made possible by the stupidity that overlooks important avian differences. This blindness to characteristics that distinguish individuals is often what gives us our leads in thinking. Free from the distraction of accepted classification, we detect new resemblances, some trivial, but occasionally one that is important. That this fanciful quest for new groupings may be of value to us we must have enough sense to distinguish the bad from the good. But even if, on first inspection, we accept the bad, its use will manifest its futility. It is largely by the choice of such leads that originality of thought proceeds.

So, although we recognize an object when we meet with it and recall it when we meet with one of its infrequent phases or with one of the conditions that previously surrounded it, recognition and recall of classes have much in common and, like so many other categories in psychology, must be distinguished quantitatively by an arbitrary border line, drawn somewhere in the domain where they merge.

The sensory component of recognition is determined chiefly by the object that is scrutinized, and this sensory content is relatively great. In recall, the sense data are derived from associated cues to memory and constitute a smaller part of the conscious whole. Often, however, a recollection is constructed by the aid of available sense data of a congruous sort that the thinker finds at hand or himself produces. Inarticulate grunts or a somewhat haphazard murmuring of digits helps me to remember a phone number. When such digits happen to be a part of the number to be recalled I pounce upon them as I would in recognition had they been spoken by another. Thus recall may proceed by successive recognitions. There are times, of course, when a recollection bursts upon us with reasonable completeness and with little pondering.

While walking down the street I observed the traces of a scratch on my hand. I knew I had acquired it a day or so before but, think as I would, its origin completely baffled me. In my silly attempt to reconstruct the history of the scratch I glanced at my hand occasionally, but as a sensory cue it was

inadequate. I reviewed my last two days in some detail, but the vagrant thought would not come to mind. Then, as I crossed a street, my eye alighted upon a child carrying a Chinese umbrella. It was such an umbrella that had scratched me and for whose memory I had been searching.

If all occurrence of recall could be shown to rest upon such conspicuous sensory memoranda as the Chinese umbrella, we would soon have to abandon the theory of thought-generated thought. But many a recollection comes to us at a time when we have no knowledge of any sensory cue that might have fathered it. Shall we say that we sometimes remember when no sensory cue is present? This is a dangerous position, for very often a little search will reveal the physical event without which the apparently spontaneous idea might never have occurred.

Those who believe that thought does not occur without cerebration, that cerebration is a transmission of impulse or some conversion of energy, that the sense organ and not the central nervous system is the starting point of any impulse, no matter what energy may or may not be picked up along the way, and that external physical force is required for receptor activity—these may go a step further. Stimuli, they will say, are necessary to thought. Will any stimulus do? If the proper set-up is temporarily present in the cortex, will the sound from the next room serve as well as the light from the window to bring about a given idea? Most people would say no, judging from the principles of association and conditioning. We know that a situation that once before accompanied the thought is likely to produce it again, and we can never exclude the possibility that all our thoughts have this dependence upon historically connected situations. The more we analyze experience the more does thought seem to have this dependence. And yet there are some events that are ubiquitous, some stimuli that have acted during the span of nearly every idea. Such are joint pressure, muscle tension, respiratory and cardiac movements, bladder distension, and many other organic states acting upon us chemically or mechanically. Whatever the vagrant thought may be, some such present

stimulus was acting during the last occurrence of that thought. Not every thought has its own specially reserved stimulus, and we can hardly believe even that every thought has its private stimulation pattern.

Together with these frequently acting stimuli there is the cortical set-up of the moment to be considered. This variable may well account for the production now of one idea and again of another, when our surroundings and our corporeal states are twice the same. This neurological variable corresponds to the conscious content that every idea trails behind it.

What, then, may properly be called a cue to memory? Shall we say that any stimulation that sets off an *amount* of nervous energy necessary for thinking is a cue, or must the energy be liberated along one of a limited number of proper pathways? The latter is, of course, true. And when we cudgel our brains for a missing memory we do two things. We keep in mind the conscious content of the desired thought, and in doing so make all the minimal movements of speech and gesture that are the co-results with thinking of the thought-producing stimuli. These movements serve as cues, for they previously occurred along with our vagrant idea. We also, in a trial and error fashion, pace the floor, roll our eyes, and take up various postures that haply in some combination or other were the thought's past accompaniment. Racking our brains is really racking our bodies. We search our environment with eyes and ears for some suggestive sign. We seek some intimation of the missing idea by ranging about in the idea's conscious context. But the *effort* of recollection appears to be one with the posturing of the baffled thinker as he unwittingly seeks a kinæsthetic hint from his joints, or holds himself immobile by way of avoiding kinæsthetic distraction during the gamut of his minimal movements.

Mr. Edwin Guthrie has spoken of the vanishing of dream memories when, upon awakening, the dreamer changes his posture. The original posture seems to furnish stimuli for recalling the dream imagery, and the new posture seems to bring to mind distracting ideas.

Both recognition and recall are perceptions, one generally

more sensate and the other less so. The sensory cue patterns are more limited in number and of greater duration for one than for the other. So we might distinguish recognition and recall, though not definitively, in terms of the amount, duration, and specificity of the stimuli that excite them. An illusion is a false recognition but a legitimate recall. By what standard do we so classify an illusion? Obviously it is by the standard of later experience. It transpires that the prowler is only a scarecrow, that the highball is iced tea, that the snake is an artifact. But we thought what we thought.

If we recognized only individuals and not classes, then an historian who had kept a chronicle of any particular object could verify its recognition, and recognition could be defined historically. But we recognize classes.

The feeling of familiarity is not the difference we seek, for this attaches to both recognition and recall. The conscious content cut loose from our knowledge of objective stimuli is a sorry field in which to find our distinction. Shall we look into the domain of behavior for a means of distinguishing the two terms? Let us consider an example. I am said to recall my latch key on occasions when it is not with me and I turn back to get it. I recognize it when I choose it from among the others and insert it in the lock. But again, this is the perception of an individual, not a class. By disregarding consciousness we gain nothing. All our difficulties remain with us. As before, our response is to a present situation. In recognition, this situation is more epecially reserved for the response, it more often endures throughout the response, and it constitutes a greater proportion, perhaps, of the physical forces that elicit the response, than in the case of recall.

May we say that recall has to do with situations not now experienced and that recognition concerns the present? It has been pointed out that both forms of response depend upon present stimulation. Is the difference to be found in a dissimilarity of setting of recall and recollection cues? Certainly the setting of the recall cue is the more variable as a rule, but not always. If I hear a concerto or see a picture or read a verse, these being unfamiliar to me, and know that it is Bach

or Rubens or Shelley, recognition occurs. I infer the historical fact of authorship from acquaintance with a class. Even a recognition of mere music, painting, or poetry is far more dogmatic than the recognition of my hat or my grandmother. To say that the cue to my recognition is a significant part of the situation that I recognize is no more than saying that the cue belongs to the class of things perceived. But classifications are usually arbitrary, vague, and peculiar to the individual. When we perceive a relationship such, for example, as that tonight we are invited to dinner, is it a recall or a recognition? It is certainly recognition when we first open the invitation and it is the same while we are seated at dinner. Is it only a recollection during the interval? Of course not. Here our two terms may often be used interchangeably. Do we recall or recognize according as we *say* we do? A good behaviorist might think so.

It may be that we have recognition when the sense datum from the cue fuses with the resulting mental state, and that we have recall when this fusion does not occur. But I think this fusion always occurs. Are not all ideas perceptions? I can believe no alternative.

Whatever we admit concerning the propriety of classifying mental events in terms of their physical setting or according to mental sequelæ yet to be experienced, it is profitable to consider the similarity of recognition and recall *per se*. Anyone can give examples of recognition and recall that all of us would agree upon. It is the borderline that is obscure.

A FURTHER STUDY OF THE DEVELOPMENT OF BEHAVIOR IN VERTEBRATES EXPERIMENTALLY REMOVED FROM THE INFLUENCE OF EXTERNAL STIMULATION

BY LEONARD CARMICHAEL

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In a previous paper[1] certain preliminary results were presented which were derived from an experimental study of the development of organisms artificially removed from the influence of external stimulation. Criticisms of this paper which have been kindly offered have shown that the facts there presented were not readily comprehended in their full systematic setting. In an effort to make the setting of the problem more clear, therefore, the present paper is divided into three sections: (I) A presentation of the current status of the problem of the development of behavior and a criticism of relevant experimental work. (II) A report of the results of a new series of experiments. (III) A theoretical evaluation of the significance of the findings reported.

PART I. HISTORICAL AND CRITICAL

As typical of the pioneer experimental work aimed to determine that which is native and that which is inherited in behavior may be taken the investigations of D. A. Spalding.[2] In his studies Spalding carried on a number of experiments, one of which was the hooding of chicks just as they came from the shell. The birds were kept thus blinded for several days. The hoods were then withdrawn and the initial pecking re-

[1] Carmichael, L., 'The Development of Behavior in Vertebrates Experimentally Removed from the Influence of External Stimulation,' PSYCHOL. REV., 1926, **33**, pp. 51–58.

[2] Spalding, D. A., 'Instinct, with Original Observations on Young Animals,' *Macmillan's Mag.*, 1873, **27**, pp. 282–293 (also reprinted in the *Pop. Sci. Mo.*, 1902, **61**, pp. 126–142). See, also, *Idem*, 'Instinct and Acquisition,' *Nature*, 1875, **12**, pp. 507 *f.*

action noted. The accuracy of the chicks on being thus released was 'marvelous at the very beginning.'[3] He noted, nevertheless, that "most frequently, however, they struck five or six times, lifting the head once or twice before they succeeded in swallowing the first food."[4] Spalding also studied other birds and certain mammals. For example, he confined recently-hatched swallows in a small box in such a manner that they could not fly. At an age when similar birds were flying the individuals of the experimental group were liberated. The birds flew off at the first trial but their flight was not absolutely perfect. Certainly, too, in the box in which they developed they could have gained much wing exercise.[5] This work of Spalding has been very widely quoted.[6] At least by the earlier writers it was often extravagantly praised. James characterizes it as 'wonderful' and Romanes says, "Spalding in his brilliant researches on this subject has not only placed beyond all question the falsity of the view 'that all the supposed examples of instinct may be nothing more than cases of rapid learning, imitation and instruction,' but also

[3] *Op. cit.* in *Macmillan's Mag.*, p. 284.

[4] *Ibid.*, p. 284.

[5] *Op. cit.* in *Nature*, p. 507.

[6] Among the many who have made reference to Spalding's results may be noted the following:

Preyer, W., 'The Mind of the Child,' Part I., 1890, pp. 336 *ff*.

James, W., 'Principles of Psychology,' 1890, II., pp. 396 *ff*.

Romanes, G. J., 'Mental Evolution in Animals,' 1883, pp. 161–165, 170–171, 175, 213, and 216.

Morgan, C. L., 'An Introduction to Comparative Psychology,' 1894, pp. 202 and 208.

Idem, 'Animal Life and Intelligence,' 1895, pp. 395 *ff*.

Mills, W., 'The Nature and Development of Animal Intelligence,' 1898, p. 261.

Baldwin, J. M., 'Development and Evolution,' 1902, p. 139.

Holmes, S. J., 'The Evolution of Animal Intelligence,' 1911, p. 99.

Thorndike, E. L., 'Animal Intelligence,' 1911, pp. 163–165.

Breed, F. S., 'The Development of Certain Instincts and Habits in Chicks,' *Behav. Monog.*, 1911, 1, pp. 2 *ff*.

Shepard, J. F., and Breed, F. S., 'Maturation and Use in the Development of an Instinct,' *J. Animal Behav.*, 1913, 3, pp. 283 *ff*.

Watson, J. B., 'Behavior, An Introduction to Comparative Psychology, 1914, p. 146.

Hunter, W. S., 'General Psychology,' 1919, p. 166.

Smith, S., and Guthrie, E. R., 'General Psychology in Terms of Behavior,' 1921, pp. 139 and 143.

Allport, F. H., 'Social Psychology,' 1924, pp. 44 *ff*.

proved that a young bird or mammal comes into the world with an amount and nicety of ancestral knowledge that is highly astonishing." [7]

That these experiments of Spalding did not 'put beyond all question' the view that behavior is individually acquired is shown by the results of later and much more carefully controlled experiments. Breed, and later Shepard and Breed,[8] have shown that the chicks' initial accuracy of pecking, proclaimed and emphasized by Spalding, is not verifiable. Craig has also shown that certain birds must learn to drink.[9] Many other objections to Spalding's results have been raised. Most significant in refutation of the claim that Spalding's experiment settled once and for all time the question of the relationship between heredity and environment is the fact stated tersely by Breed that "the early post-embryonic life of the chicks continued the scope and activities already begun in the egg." [10] Strangely enough Spalding himself knew of this objection, but he waives it aside. He quotes and adversely criticizes the following statement made by Helmholtz, which might well have been made by a modern 'anti-instinct' psychologist: "The young chicken very soon pecks at grains of corn, but it pecked while it was still in the shell, and when it hears the hen peck it pecks again, at first seemingly at random. Then when it has by chance hit upon a grain it may, no doubt, learn to notice the field of vision which is at that moment presented to it." [11]

It is this very fact, which cannot, indeed, be emphasized too strongly, namely, that behavior does begin before birth or hatching, that makes the major contentions of these experiments invalid.[12]

[7] Romanes, G. J., *Op. cit.*, p. 161.

[8] In the articles cited above.

[9] Craig, W., 'Observations on Doves Learning to Drink,' *J. Animal Behav.*, 1912, 2, pp. 273–279.

[10] Breed, F. S., *op. cit.*, p. 75.

[11] Spalding, D. A., *loc. cit.* in *Nature*.

[12] For experimental work calculated to throw light upon the prenatal responses of organisms, *cf.* Preyer, W., 'Die Seele des Kindes,' 1884 (translated 1890, pp. 212 *ff*); Minkowski, M., 'Zum gegenwärtigen Stand der Lehre von den Reflexen,' Neurologische und psychiatrische Abhandlungen aus dem Sweizer *Arch. f. Neur. u. Psychiat.*, 1925,

Even the carefully controlled experiments of Breed and of Breed and Shepard while demonstrating many important facts in regard to development are also open to this objection. A bird that is hatched or an animal that is born is already environmentally conditioned.[13] Moseley's experiments upon similar material are even less calculated to determine the relative parts of heredity and environment in determining the initial performance of the actions which are later called pecking. In her experiments the chicks which were studied were given ample opportunity for exercise save that they were kept in the dark.[14]

Bird, another recent student of this subject, on the contrary, notes that the increase in the pecking 'instinct,' shown graphically in so many studies, may not be the specific maturation of any specific synaptic connections. He shows that the early 'maturation' of pecking may be but a 'general improvement in motor control.' [15] Moreover, this same experimentalist holds that after this initial period "practice and habit formation are adequate explanatory concepts of the perfection of the swallowing reactions of the chick." [16] In a later paper, however, Bird makes it very clear that his observations on the initial specificity of the behavior of young chicks make him far from a supporter of Kuo's contention that the conditioning of the post-natal environment is a sufficient explanation of the origin of behavior patterns.[17]

Heft I; Lane, H. H., 'The Correlation Between Structure and Function in Development of the Special Senses of the White Rat,' A Dissertation, Norman, Oklahoma, 1917; Paton, S., 'The Reactions of the Vertebrate Embryo and the Associated Changes in the Nervous System,' *Mitt. a. d. Zool. Stat. z. Neap.*, 1907, **18**, pp. 535–581; *Idem*, (second paper) *J. Comp. Neur.*, 1911, **21**, pp. 345–372; Brown, T. G., 'On the Activities of the Central Nervous System of the Unborn Fœtus of the Cat, with a Discussion of the Question whether Progression (Walking, etc.) Is a 'Learnt' Complex,' *J. Physiol.*, 1915, **49**, pp. 208–215. Also the at present unpublished work of Avery, G. T., and Stone, C. P., on congenital behavior exhibited by prenatally delivered guinea-pigs.

[13] The importance of this conception for human psychology has been made very explicit by J. B. Watson, 'Behaviorism,' 1924, pp. 88 *ff.*

[14] Moseley, D., 'The Accuracy of the Pecking Response in Chicks,' *J. Comp. Psychol.*, 1925, **5**, pp. 75–97.

[15] Bird, C., 'The Relative Importance of Maturation and Habit in the Development of An Instinct,' *Ped. Sem.*, 1925, **32**, p. 81.

[16] *Ibid.*, p. 91.

[17] *Idem*, 'The Effect of Maturation upon the Pecking Instinct of Chicks,' *Ibid.*, 1926, **33**, p. 219.

Obviously, the complicated activity of the bird in liberating itself from the egg cannot be ignored.[18] Even this behavior, however, is really an advanced stage in the embryonic activity of the organism.[19] Elsewhere I have attempted to show that there is no theoretical compulsion to believe that even the very early actions of the embryo in the egg or in the uterus are purely native.[20]

While valuable in other respects, the conclusions of Yerkes on the heredity of savageness and wildness in rats, and of Yerkes and Bloomfield on the hunting activities of kittens do not show that the specific motor acts noted occurred for the first time in the behavior observed.[21] Indeed, as these latter writers admit, 'racial and individual experience' early become so 'completely intermingled' that separation is, to say the least, difficult.[22]

The studies of the development of sex behavior are significant, also, rather as an example of the integration of elementary acts into a complex and comparatively unitary behavior pattern than as a demonstration of the native origin of the part-acts themselves.[23] The 'abnormalities' of the sex function discovered in animals raised under peculiar conditions show the essential part played by the environment in the development of these activities.[24]

The great difficulty, moreover, of pointing to any satisfactory criterion of the innate is another factor that has led to

[18] Craig, W., 'Behavior of Young Birds in Breaking out of the Egg,' *J. Animal Behav.*, 1912, 2, pp. 296–298.

[19] Paton, S., 'Experiments on Developing Chicken's Eggs,' *J. Exper. Zool.*, 1911, 11, pp. 469–472.

[20] Carmichael, L., 'Heredity and Environment: Are they Antithetical?' *J. Abn. & Soc. Psychol.*, 1925, 20, pp. 245–260.

[21] Yerkes, R. M., 'The Heredity of Savageness and Wildness in Rats,' *J. Animal Behav.*, 1913, 3, pp. 286–296, and Yerkes, R. M., and Bloomfield, D., 'Do Kittens Instinctively Kill Mice'? Psychol. Bull., 1910, 7, pp. 253–263.

[22] *Ibid.*, p. 263.

[23] *Cf.* Stone, C. P., 'The Congenital Sexual Behavior of the Young Male Albino Rat,' *J. Comp. Psychol.*, 1922, 2, pp. 95–153, and *Idem*, 'The Awakening of Copulatory Ability in the Male Albino Rat,' *Amer. J. Physiol.*, 1924, 68, pp. 407–424.

[24] For a discussion of the point and references to the experimental literature see Allport, F. H., 'Social Psychology,' 1924, pp. 69 *ff*.

the partial or absolute rejection of the concept of heredity as a determiner of conduct.[25]

Even McDougall admits that from the standpoint of what he terms 'the mechanistic hypothesis' the conception of inherited behavior must be rejected. He holds that instinct, for example, cannot intelligibly be held to consist of more or less compound reflex action. The motor mechanisms employed in instinctive acts, he avers, are, at least in some measure, made up of responses that have been individually acquired and previously practiced.[26]

This criticism is one that must be heeded by the scientific student who accepts the mechanistic principle as a working hypothesis. If an act is to be declared *hereditary* it must be shown to occur for the first time in a manner that is independent of environmental conditioning.

It is to meet this challenge that the present series of experiments were devised. In the experiments already reported, as well as in certain earlier work by previous investigators there cited, it is shown that it is possible to allow organisms to develop under an anæsthetic such that morphological growth occurs in the absolute absence of observable external movements. Such organisms are allowed to grow, so that externally they appear better developed than a similar group of normal organisms when they began swimming. At this stage the experimental animals are released from the anæsthetic in which they have developed. Thus, after a period of complete inactivity, the experimenter can record the time necessary for the very first response to develop in the organisms. This time is shown to be surprisingly short.

What process is occurring during the short period after the organism is removed from the anæsthetic and before it makes any observable response to the continual external stimulation

[25] For a bibliography of this so-called anti-instinct controversy see my paper, *op. cit.* in the *J. Abn. & Soc. Psychol.*

[26] McDougall, W., 'The Use and Abuse of Instinct in Social Psychology,' *J. Abn. & Soc. Psychol.*, 1921–1922, **16**, pp. 310*f*. With the alternative vitalistic principle now held by this writer we are not concerned in this paper. He holds that an innate instinct with all its hormic phylogenetic potency may make use of any motor mechanisms however developed and still constitute inherited behavior.

to which it is subjected? Is this a period of rapid neural change? Is it a period of exceedingly rapid learning? Or, is it merely a period of time necessary to remove the masking effect of the anæsthetic? If answers can be given to these questions it will be possible to state with some assurance whether or not 'preformed pathways' exist which are ready to function before they have been exercised. Even, however, if it is demonstrated that such pathways do exist it will by no means be equivalent to saying that these pathways are the result of the mere innate maturation of certain germinal determiners and that they have come into existence in independence of the environment. An alternative and seemingly more satisfactory position than this will be considered in the last section of the present paper.

Part II. Experimental

Unlike the previous series of experiments in which the embryos of both the frog (*Rana sylvatica*) and the salamander (*Amblystoma punctatum*) were employed, in the present series only amblystoma were used. This selection was made because of the size of the organisms and because previous trials had shown them to respond satisfactorily to an anæsthetic. This type, moreover, is rendered peculiarly available because of the fact that its embryological development has been most thoroughly studied. Coghill has made remarkable and detailed experimental studies of many phases both of the anatomical and physiological development of this organism.[27]

The eggs used in the present series of experiments were secured in the same locality previously reported and the technique of preparation was the same as that already described.

At a period of morphological development well previous to the point at which any apparent movement occurs, the organisms were placed in individual glass development dishes.

[27] For the student of behavior this series of papers is most significant. They range from 1902 to the present. Particular reference should be made to a group of papers on 'Correlated Anatomical and Physiological Studies of the Growth of the Nervous System of Amphibia,' (Nos. I. to V.) *J. Comp. Neur.*, 1914, 24; *Ibid.*, 1916, 26; *Ibid.*, 1924, 37; *Ibid.*, 1926, 40.

These dishes were then divided into two groups. One set, the *control group*, was filled with tap water while the other, the *experimental group*, was filled with an anæsthetic solution of the desired strength. Chloretone was again used as the anæsthetic. The typical strength of the solution, save where it was varied as noted below, was four parts of chloretone in 10,000 parts of water.

The experimental work was carried on in a room which maintained as nearly as possible an even temperature. No especial effort, however, was made to regulate either this factor or the light in the room as both the experimental animals and the control group were at all times subject to the same conditions. The development of the drugged and the normal embryos as judged by external appearance was quite similar. In all cases, however, development seemed to be somewhat more slow in the anæsthetized group. Possibly because of improved ability on the part of the experimenter in removing the protective 'jelly' of the eggs, fewer abnormal individuals developed in this series of experiments than in those previously reported.

Soon after the larvae in the control dishes began to respond to the stimulation of a light touch with a blunt rod, they became free swimmers. The drugged embryos during this period showed absolutely no response to stimulation, nor in any case did they respond before they were released from the anæsthetic.

When the drugged organisms showed a morphological development in advance of that shown by the individuals of the control group when they had first become good swimmers, the drugged organisms were released. This release was accomplished by removing the organisms one by one from the anæsthetic bath and placing them in tap water. As soon as they were placed in the fresh water they were subjected to continual gentle stimulation and the time was taken from the moment of immersion in the tap water until the first movement occurred. This time for the first series of the present experiment is shown in the second line of Table I. After all of these experimental animals had been released a period of

Table I

Showing Time Elapsing after Removing Embryos from the Anæsthetic Solution in which They had Developed before the First Response to Stimulation Was Noted

Embryo number	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
Time in minutes	4	4	3	6	5	5	4	2	2	4	5	4	7	3	5	5	4	5

36 hours was allowed to elapse during which the members of the experimental group normally swam about in tap water. When this period had passed, the same individuals (they were at all times kept distinct in numbered dishes) were re-anæsthetized in a solution of the same concentration in which they had developed. The time required for observable response to stimulation to cease after re-immersion in the drug solution is shown in the second line of Table II. The organisms were allowed to stay in this solution once again in a condition of apparently complete inactivity for twenty-four hours. At the conclusion of this period they were individually removed from the drug and placed in tap water. The time required after this second immersion in fresh water for response to occur is shown in line three of Table II.

Table II

Showing Time in Minutes Required for Re-anæsthetizing Embryos Noted in Table I. and Time Elapsing after They were Removed from the Anæsthetic the Second Time and before the Second Response Was Observed

Embryo Number	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
Time required for re-anæsthetizing	1	3	1	2	2	3	1	2	2	1	1	2	3	1	1	1	2	2
Time to emerge from second anæsthetic	5	4	6	5	5	5	3	4	5	5	3	4	8	4	4	6	6	6

In Table III are shown the results of a check experiment in which certain of the organisms of the control group which had developed normally in tap water were anæsthetized after they had been free swimmers for several days.

In order to determine if possible the rôle of the anæsthetic in determining the time required for arousal from narcosis

Table III

Showing the Time in Minutes Required for Anæsthetizing Normal Embryos Raised to Free Swimming Stage in Fresh Water. Also Showing Time Required for First Response to Occur after their Removal from Anæsthetic

Embryo number	19	20	21	22	23	24	25	26
Time for anæsthesia	2	1	2	2	2	2	2	1
Time for elimination of anæsthetic	7	8	11	8	6	8	10	8

certain individuals were placed in chloretone solutions of varying strengths. For the most part these experiments were failures. The increased strength of the drug killed the organisms. A few, however, of the free swimming larvæ placed in a solution of 8 parts of chloretone in 10,000 parts of water developed in such a manner that the time required for the release could be taken. The times required for the complete anæsthesis (as shown by the cessation of response to external stimulation) and for subsequent arousal are shown in Table IV.

Table IV

Time Required in Minutes for Inducing Anæsthesia and Subsequent Arousal in Previously Free-swimming Organisms Placed in a Strong Anæsthetic Solution of 8 Parts of Chloretone in 10,000 Parts of Water

Embryo number	27	28	29	30
Time to induce anæsthetic state	.5	.5	.4	.2
Time for release	30	10	18	33

The conclusion of the present experiments is that there is little significant difference in the time required for the very first observable response on release for the anæsthetic in an organism that has never before shown movement and in one that has been free moving, re-anæsthetized, and released the second time. This evidence together with that shown in Table IV seems to point to the nature of the process that occurs between the time of the immersion of the anæsthetized organism in fresh water and its first movement. This process appears to be no more rapid than is the elimination of the

masking effect of the anæsthetic upon organisms which already have well integrated response mechanisms. It may, therefore, safely be assumed that the time required for release is not a period of learning but rather an interval required for the removal of the masking effect of the anæsthetic.

Part III. Theoretical

At first sight these results, therefore, seem to give confirmation to the hypothesis that the neuromuscular mechanism upon which behavior depends is developed in the individual by a mere maturation of innate determiners. This view is commonly held by a number of physiological psychologists.[28] The results presented here are not conclusive enough, the present writer believes, to merit the acceptance of the radical form of the maturation hypothesis. It seems true, however, that the findings of the present experiments give better evidence for this view than has at times been taken as completely satisfactory by the nativistic psychologists.

The question may then be asked: If the reflex mechanism upon which the first responses of amblystoma depend are not developed by mere maturation, how is it that they are ready to function the first time they are effectively stimulated in an organism that has just been released from an anæsthetic in which it has developed from an early and immobile stage?

Before this question may specifically be solved for the particular type under consideration it may be well to consider in outline the general status of the problem of development. An answer, indeed, to this question may only be given by a study of the dynamic process of embryological development. One who accepts the strict maturation view must show how,

[28] W. S. Hunter says, for example, ('General Psychology,' 1919, pp. 166*f*): "Undoubtedly as the chick grows older the nerve centers (synaptic connections) which control the instinct are maturing so that part of the increase in efficiency is due to the growth of the instinct as a result of strictly inherited tendencies. This has been tested and proven to be the case by Shepard and Breed, 1914." And E. L. Thorndike says ('Educational Psychology,' Vol. 1, pp. 1 *ff.*) "Any man possesses at the very start of life—that is, at the very moment when the ovum and spermatozoön which are to produce him have united—numerous well defined tendencies to future behavior. Between the situations which he will meet and the responses which he will make to them, preformed bonds exist."

by heredity alone, such hypothetical entities as Thorndike's 'preformed bonds' of the germ cell become the real neural connections determining the behavior of the adult. The biologists who are interested in heredity, have displayed far too little interest, as Pearl has shown, in this all important process of somatogenesis.[29] The criticism may be urged with even greater force against the nativistic psychologists for these writers have almost completely ignored the developmental embryology of the structures that they posit as the basis of innate behavior. And yet, if *maturation* is to be more than a vitalistic shibboleth, the process by which it occurs must at least be suggested. Otherwise the growth of the neuromuscular mechanisms upon which native behavior is supposed to depend must seem to result from some bold necromancy comparable to the erection of the pleasure palaces of Eastern romance which only become a theatre of activity after their magical construction.

Those familiar only with biological literature, and who have not studied the writings of the nativistic psychologists may think that the present criticism is an attack upon a man of straw. There can be no doubt, however, as I have shown elsewhere,[30] that unmitigated preformism still permeates much psychological writing. This view certainly must be modified for as Professor Jennings has recently said: "Or more properly, characteristics are not inherited at all; what one inherits is certain material that under certain conditions will produce a particular characteristic; if those characteristics are not supplied, some other characteristic is produced." [31]

The suggestion has been made that the term *heredity* be held to include the normal environmental influence, but to the present writer this seems like solving a very genuine physiological problem by a verbal trick.[32]

If the hypothesis that the organism is equipped with certain preformed structures, which develop without the aid

[29] Pearl, R., 'Modes of Research in Genetics,' 1915, pp. 2 *ff.*

[30] Carmichael, L., *loc cit.* in *J. Abn. & Soc. Psychol.*

[31] Jennings, H. S., 'Prometheus,' 1925, p. 43.

[32] *Cf.* Wells, W. R., 'The Meaning of "Inherited" and "Acquired" in Reference to Instinct,' *J. Abn. & Soc. Psychol.*, 1922, 17, pp. 160 *ff.*

of the environment be rejected, it seems equally true that the contention that all behavior may be explained as the result of post-natal conditioning is equally in error. Strangely enough, however, there are psychologists, such as Kuo, who make this assertion.[33] The whole gradual process of building up behavior patterns in pre-natal life to which reference is made in the early part of this paper is sufficient evidence against this view.

Beside these two radical views of psychological preformism and psychological epigenesis there is of course a middle view which the present writer has defended in the past and which it seems must implicitly be held by most students of this problem. This hypothesis holds that *all* behavior is the result of the *interdependent*, but not necessarily equal activity of certain factors which may be classed as hereditary and those factors which may be classed as environmental. According to this view the intricate development of receptors, nerve trunks, central apparatus, and motor end-organs appears, not as a teleological preparation for the future, but as the result of the truly mechanistic determination carried out by means of a functional stimulation and response within the organism itself.[34]

In the light of the above discussion the specific problem of this paper may now be considered. In the work here reported it is shown that a neuromuscular apparatus that had never before functioned as a response mechanism is capable of determining external behavior the very first time that it is effectively stimulated. To the present writer this seems to point to the fact that this mechanism had been developed by certain processes of a stimulus-response nature within the organism itself.[35] The old view that functional activity is

[33] Kuo, Z. Y., 'A Psychology without Heredity,' PSYCHOL. REV., 1924, 31, pp. 427–447.

[34] This problem has been treated in an original manner that is slightly different from the view presented here by R. M. Ogden, 'Crossing "The Rubicon Between Mechanism and Life"', *J. Phil.*, 1925, 22, pp. 281–293.

[35] Much is known regarding the nature of these processes, but as yet there is no unanimity of opinion upon them by the students best qualified to judge. Chemical, electrical, and vibratory theories have been advanced to account for the development

dependent upon myelinization has recently been revived, but this too is still in question.[36]

In summary and conclusion it may be said that the experiments reviewed in the first section of the paper do not show what behavior is native and what behavior is acquired. Even the experiments here reported, in which the very first external movements are studied, do not give final evidence upon this question. It has been shown, nevertheless, that the time elapsing after the removal of the organism from the anæsthetic before the first movement occurs is probably the time necessary for the elimination of the narcotic and not a period of functional development. Theoretically it is pointed out that these results seem still to point to the conclusion presented in previous papers, namely, that the development of the behavior mechanism is not alone dependent upon heredity or environment, but that it is the result of the *interdependent* action of both of these factors.

and interrelationship of nerve cells. *Cf.* Herrick, C. J., 'Neurological Foundations of Animal Behavior,' 1924, pp. 111 *ff.* and the references there cited. See also, Weiss, P., 'The Relations between Central and Peripheral Coordination,' *J. Comp. Neur.*, 1926, 40, pp. 241–251. As I have elsewhere suggested the hypothesis of Bok and Kappers as developed in the conception of *neurobiotaxis* seems most satisfactory. This view in any event scarcely merits the curt dismissal which it receives from Lashley. (*Cf.* Lashley, K. S., 'Studies of Cerebral Function in Learning,' VI, Psychol. Rev., 1924, 31, p. 369).

[36] Tilney, F., and Casamajor, L., 'Myelinogeny as Applied to the Study of Behavior,' *Arch. Neur. & Psychiat.*, 1924, 12, pp. 1–66. This paper is an experimental study which aims to connect the possibility of behavior with the sheathing of the neurons. This 'myelinogenetic law' was at one time a favorite resort of the nativistic psychologists (*cf.* the long series of papers by Flechsig, references given by Tilney and Casamajor, *loc. cit.*) Hall made use of it. (*Cf.* Hall, G. S., 'Adolescence,' 1904, 1, p. 109.) On the basis of Watson's experimental work ('Animal Education,' 1903, pp. 1–122) it was generally abandoned. Indeed, Thorndike has said of the law that it seems 'gratuitous and improbable,' ('Educational Psychology,' 1913, 1, p. 229). To the present writer there seems little in the new work cited above to belie the assertion of Thorndike.

THE PRESENT STATUS OF THE TENDENCY TO COLLECT AND HOARD

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Undue emphasis upon periodicity in growth has resulted in the more important characteristic of growth, namely, its continuity being obscured or underestimated. Of periodicity and rhythm in growth there can be no doubt. But any thoughtful attempt to characterize a particular period must bring the conviction that each stage merges into the succeeding one and that the obvious characteristic traits of each period have their beginnings in preceding stages and merge gradually into succeeding ones.

The student of psychology or education is cognizant of the various attempts to characterize certain periods in the child's development. Team play and social participation in play behavior have been emphasized as characteristic of certain periods of development. Individualistic play too has been designated to be characteristic of other periods of development. Collecting and hoarding are individualistic activities often regarded as instinctive and characteristic of particular periods of development. Thorndike says, "There is originally a blind tendency to take portable objects which attract attention, and carry them to one's habitation. There is the further response of satisfaction at contemplating and fingering them there. These tendencies commonly crystallize into habits of collecting and storing certain sorts of objects whose possession has additional advantages, and abort as responses to other objects whose possession brings secondary annoyances" (1).

The writers will avoid raising the question of the instinctive character of collecting and hoarding. They have observed certain clear evidence of *less* marked interest in these activities

among present-day children than was reported a few decades ago. Other activities seem to have become vastly more popular than these. The emphasis placed upon these activities by educational theorists makes it desirable to estimate their present importance in the development of the child.

The following paper presents (1) the frequency with which children now engage in collecting and hoarding activities at various ages; (2) comparison of the results now obtained with those of earlier investigations.

Method

Over 5,000 children were asked to check from a list of play activities those in which they had voluntarily engaged during the preceding week. The directions were specific, the teachers were instructed in giving the tests, and the data were assembled from unselected pupils in order that the results of the investigation might be reliable. Data were secured from children in Kansas City, Kansas, Lawrence, Bonner Springs, and Moran, Kansas. Individuals included in this investigation of ages 17½ to 22 were for the most part students of the University of Kansas.

In order that seasonal differences might be taken into account the list was checked by the above groups on each of three different dates, November 7, 1923, February 20, 1924, and April 30, 1924. The same list of activities was checked on each of these dates and the same procedure in administering the test was used at each testing.

In order to discover differences between rural children and city children the list was similarly checked in certain one-teacher, rural schools, each having an average attendance of less than twenty-five pupils. These rural schools were located in Shawnee, Douglass, and Franklin counties (Kansas). Owing to the difficulties involved in administering the list to these rural pupils, the quiz was not administered three times during the year. The pupils checked the list in the fall of 1924. To verify the initial findings the rural pupils were again asked to check the list in the fall of 1925. Additional data were secured from 6,000 pupils of Kansas City, Missouri,

in January of 1926, and the reliability of the earlier findings further verified. The total number of pupils from whom data were obtained is indicated in Table I.

Table I

Number of Pupils Included in the Present Study

Ages	Nov. 1923	Feb. 1924	Apr. 1924	Nov. 1924 (Rural)	Nov. 1925 (Rural)	Jan. 1926 (White)	Jan. 1926 (Negro)
			Boys				
8½	98	90	80	89	35	100*	39
9½	169	161	144	102	45	100	75
10½	182	169	160	102	67	100	94
11½	187	167	184	106	65	100	89
12½	249	201	176	101	79	100	104
13½	280	231	259	93	70	100	80
14½	274	252	238	67	50	100	48
15½	230	247	247	46	22	100	40
16½	210	181	193	—	—	100	—
17½	145	130	146	—	—	68	—
18½	115	170	130	—	—	22	—
19½	95	119	102	—	—	—	—
20½	50	73	59	—	—	—	—
21½	53	57	43	—	—	—	—
22	79	105	68	—	—	—	—
			Girls				
8½	100	97	99	85	39	100*	48
9½	174	139	144	85	49	100	90
10½	215	199	176	103	53	100	123
11½	235	222	220	109	64	100	114
12½	326	289	266	83	69	100	110
13½	269	235	263	89	57	100	91
14½	301	282	278	68	35	100	71
15½	261	244	256	27	21	100	34
16½	251	223	235	—	—	100	—
17½	182	208	193	—	—	50	—
18½	120	174	167	—	—	—	—
19½	101	115	93	—	—	—	—
20½	73	124	76	—	—	—	—
21½	41	85	66	—	—	—	—
22	44	114	88	—	—	—	—

* Data were assembled from a much larger number of white children in Kansas City, Missouri, but it was decided that 100 cases would be sufficient to provide a fair sampling of the returns.

One of the items included in the list was No. 38, 'collecting stamps, birds' eggs, etc.' From responses to this item, the writers hoped to secure significant data in regard to the extent to which pupils now engage in such activities.

Frequency of Participation in Collecting and Hoarding Activities

Figure 1 and Table II show the percentages of boys and girls of various ages who indicated that they had engaged in collecting activities during the preceding week. The percentages are the averages of the several investigations. Fig. 1 shows that interest in collecting stamps, birds' eggs, etc.,

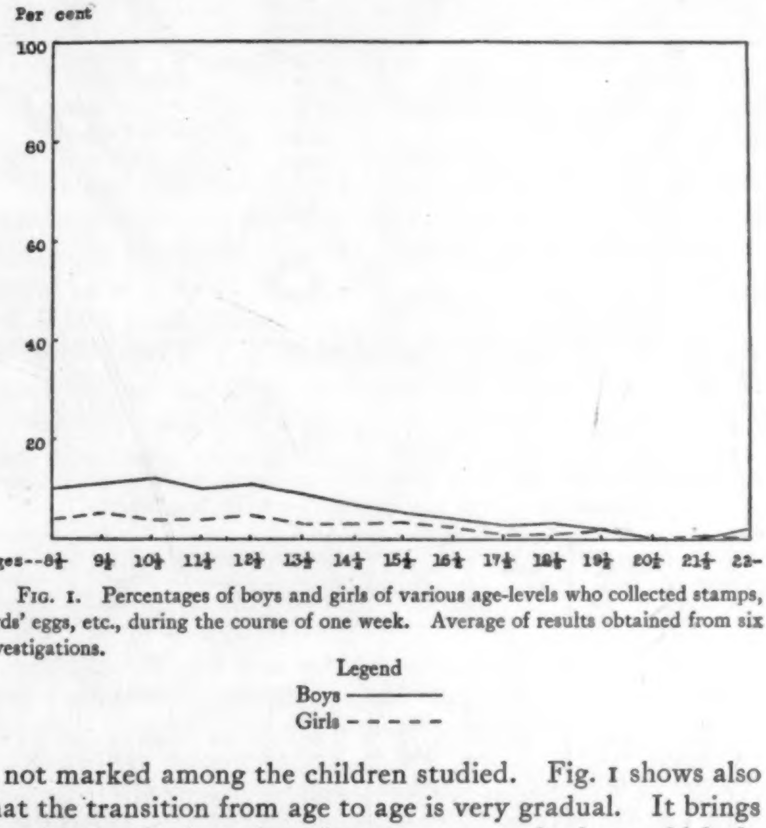

Fig. 1. Percentages of boys and girls of various age-levels who collected stamps, birds' eggs, etc., during the course of one week. Average of results obtained from six investigations.

Legend
Boys ————
Girls - - - - -

is not marked among the children studied. Fig. 1 shows also that the transition from age to age is very gradual. It brings out clearly the fact that there are no *age-levels at which the interest in collecting and hoarding suddenly decreases or increases by spurts.*

Table III shows the percentages of boys and girls of various ages who indicated that they had engaged in making collections during the week preceding the examination. Data are

Table II

Percentages of Boys and Girls of Various Age-levels who Collected Stamps Birds' Eggs, etc., during the Course of One Week

Average of results obtained from all investigations

Ages	Boys	Girls
8½	10.35	4.40
9½	10.55	4.73
10½	12.35	4.43
11½	10.24	3.94
12½	10.69	5.06
13½	9.16	3.26
14½	7.	2.81
15½	5.	2.86
16½	4.	2.
17½	3.	1.
18½	3.	1.
19½	2.	2.
20½	0.5	0.0
21½	0.0	0.5
22	2.	0.0

presented for city and rural children separately and are further partitioned according to race.

Table III

Percentages of Boys and Girls of Various Age-levels who Collected Stamps, Birds' Eggs, etc., during the Course of One Week

Ages	Nov. 1923	Feb. 1924	Apr. 1924	Nov. 1924 (Rural)	Nov. 1925 (Rural)	Jan. 1926 (White)	Jan. 1926 (Negro)
				Boys			
8½	10	4	29	7	7	9	8
9½	11	11	17	9	10	9	4
10½	12	15	21	7	11	8	5
11½	11	8	21	2	6	11	1
12½	9	12	20	8	8	11	3
13½	9	10	14	2	6	16	1
14½	7	7	11	6	7	9	0
15½	6	4	5	7	3	6	3
16½	7	3	3	*	*	3	*
17½	3	4	3	*	*	3	*
18½	3	5	1	*	*	4	*
19½	2	2	2	*	*	*	*
20½	0	0	2	*	*	*	*
21½	0	0	0	*	*	*	*
22	3	4	0	*	*	*	*

Girls

8½......	10	4	4	4	13	1	0
9½......	7	1	7	5	8	3	3
10½......	5	5	4	10	6	2	2
11½......	6	3	4	5	12	0	1
12½......	6	6	6	9	12	1	3
13½......	1	2	6	4	9	5	0
14½......	3	3	4	4	0	0	6
15½......	2	2	5	0	0	3	3
16½......	2	1	2	*	*	1	*
17½......	0	0	2	*	*	2	*
18½......	2	1	1	*	*	*	*
19½......	3	3	1	*	*	*	*
20½......	0	0	0	*	*	*	*
21½......	0	0	0	*	*	*	*
22......	0	0	0	*	*	*	*

* No data assembled.

Marked seasonal differences are not revealed by the data in regard to the number of persons engaging in these activities. There are obviously differences in the types of activities engaged in at various times in the year. It is of interest that there is a tendency for younger boys (8½–14½) to engage more frequently in collecting activities in the April investigation than in the other investigations. This difference, not revealed in the data for the girls, is probably due to the fact that birds' eggs may be collected only at this season.

Comparison with Other Investigation

It is of interest to compare the above findings with those obtained by C. F. Burk almost thirty years ago (2). Burk makes the following statements:

> . . . the children were asked to make out a list of all the things they had ever collected, tell when they began and when they stopped any collection, give the number of objects in each, and tell also various things about them, as will be discussed later. The results proved so fertile that a set of questions was made out by the children, and a similar set was gathered from school children of Santa Rosa. Several days were allowed in order that they might have time carefully to think up, look up and count up their collections, and jog the memory of their mammas, also, as to their past collections. In some cases, as when an enterprising youth of ten years recorded sixty-six collections, fifty-five of them still continuing, the teacher herself consulted the mother and made sure that all were verified.

> Records were obtained from 510 Santa Barbara children and 704 Santa Rosa children, in all 607 boys and 607 girls, or 1214 children.
>
> The universality of the collecting interest was strikingly brought out. Only ten per cent of the boys and nine per cent of the girls were not actively making collections at the time. . . . (3).

Although Burk employed a technique differing from that employed by the writers, it is questionable that this alone is sufficient to account for the difference in the findings. Burk found ninety per cent of the children actively making collections (4). The present writers found hardly more than ten per cent doing so. Nor were Burk's subjects restricted in their interests, "That the children on the average were in process of making from three to four actual collections bespeaks a considerable amount of energy being drained off through the channels of this instinct" (5).

It is of course true that in the present study interest in stamps and birds' eggs might be revealed more readily than interest in other activities since mention of these two items in the list might serve as memory aids. Comparison of Burk's findings for these two items with the results of the present investigation, therefore, will operate to make the techniques employed more comparable. According to Burk's charts more than 40 per cent of the boys of ages 7 to 14 inclusive were actively engaged in collecting stamps. At ages 9 and 11 the per cent rose almost to 60. The childrens' interest in collecting birds' eggs was not quite so great. However, between ages 8 to 16 inclusive more than 15 per cent of the boys of each age-level were collecting birds' eggs, and from ages 12 to 14 inclusive the per cent was above 30 at each age.

In contrast to Burk's data, this study shows that at no age-level were as many as 15 per cent of boys making collections of any sort. To what is this large difference due? It seems reasonable to assume that interest in collections varies from decade to decade. But Burk seemed to be oblivious of the fact that the only permanent phenomenon is that of eternal change. Note the following conclusions:

> The marble collection begins at least by six years, with a small number of followers, and reaches its height from seven to ten years,

but especially at eight and nine years. After this age it declines, and from thirteen years on plays a small part. Stamps hold their own from seven to fifteen years, declining thereafter. The prominent ages are from nine to fourteen years. Cigar-tags are rather full-fledged at as early an age as six years. The craze increases, reaching its greatest intensity at sixteen years, and then diminishes, dying out practically at sixteen years. This collection is prominent through a greater number of years than any other collection. The bird-egg fever begins mildly at seven years and increases, reaching its height from twelve to fourteen years (6).

A further generalization from data such as Burk's is made by Norsworthy and Whitley. "The strength of this tendency (collecting) in childhood and the fact that it is still present in so many adults . . . (witness the collections of string pieces, bottles, boxes, corks, bags, hats, etc., as well as those of hunting trophies, stamps, coins, rugs, china, art objects, etc.), suggest that the schools would do well to use the instinct more" (7).

The implications revealed by the data herein presented are in sharp contrast to those presented above. Older children become less and less interested in making collections. It may be that present-day conditions present opportunities to use leisure that are more pleasing to young people than are the activities collecting and hoarding. Activities such as moving-picture theatre attendance, dancing, listening to the radio, etc., may prove vastly more attractive to the youth today than collecting and hoarding activities. The conclusion would seem to be that deductions based upon studies of children's 'interests' need to be made with much caution since such interests are often temporary ones. Both children and adults displayed much interest in 'cross-word puzzles' during the winter of 1923–24. Conclusions based upon a study of this interest would be applicable only to the time at which the study was made. The parallel with respect to collecting and hoarding activities is obvious.

In one point only does Miss Burk's study and the present study show marked agreement, namely, the age at which the collecting tendency reaches its maximum intensity. Miss

Burk found that the largest number of collections was made by ten-year-old boys. The present writers find that a slightly larger per cent of boys of this age-level were engaged in making collections. However, as mentioned above, the transition from age to age is very gradual. Certainly, a difference of one or two per cent is not enough to justify the assumption that age ten is an age of 'individualism' and that ages nine and eleven are to be characterized differently.

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THE HISTORICAL DEVELOPMENT OF COMPARATIVE PSYCHOLOGY

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Introductory.—The rapid development of comparative psychology during the past three or four decades has doubtless tended to create the general impression that nothing worthy of the name had existed previously. It is true that only within this period has the science emerged as a separate discipline and established itself in a more or less clearly defined field, with its own systematic problems and experimental methodology. It is equally true, however, that comparative psychology, like most other branches of modern science, must seek for its humble beginnings in the remote past.

The larger aim and viewpoint of the modern movement cannot be justly appreciated or fairly evaluated apart from a knowledge of its more important historical antecedents. For it seems to be true that a science can no more outlive its tradition than an individual can outlive his heredity. And few modern disciplines possess a richer heritage of opinion and discussion than comparative psychology; few have been more consistently exploited in ages past by men of such diverse interest, temperament and training. The philosopher and the theologian as well as the naturalist, both trained and untrained, have each attempted to solve the riddle of the living organism and its behavior. In tracing the effect of these various influences upon the later development of the science, we shall find more often than not that facts have been either neglected or subordinated to theoretical bias; that in spite of the great variety of thinkers who have dealt with the problems that properly belong to this field, the general solutions offered have been few; that the more outstanding viewpoints of past epochs continue to be rallying points for various schools and 'isms.'

It is manifestly impossible, within the limited space here available, to present more than the briefest sketch in a historical survey that should properly extend back to the earliest beginnings of natural history. For, as we shall see, observations of animal behavior (typical overt activities in field and stream) were probably among the first to reach the scientific level, and may very conceivably have become the nucleus around which the zoölogical aspect of natural history finally developed. It is hoped that this brief sketch may serve, at any rate, to place the modern movement in a proper historical perspective, and afford a basis for the evaluation of present-day tendencies in this field.

The following five epochs representing the most important stages of development have been recognized:

I—Ancient Animal Lore
II—The Greek Influence
III—The Early and Middle Ages
IV—From the Sixteenth Century to Darwin
V—The Modern Experimental Movement

In addition to a short general bibliography, a selected list of references for each period has been appended. In making this selection the interests of the general reader, as well as accessibility of material, have been kept in mind, and this accounts for the reference to secondary sources in so many instances. In general the bibliography has been kept within the narrowest limits consistent with even a brief systematic treatment of each period.

I. Ancient Animal Lore

Early Origins.—A sketch of the development of comparative psychology might well begin with the writings of Aristotle which include, along with a mass of general natural history material, a fairly systematic account of the mental life of the animal world as he knew it. Certainly nothing of great importance for our topic, either in content or method, appears to have developed previous to the beginnings of science among the Greek philosophers of the sixth century B.C. Neverthe-

less, it has seemed appropriate to begin this survey with a brief account of the animal lore of the pre-scientific ages, including not only the great civilizations that antedate the rise of science among the Greeks but also the earlier prehistoric cultures as well.

It is a truism to say that the animal has played an important, and often a dominating rôle in the practical and cultural life of mankind from the earliest times. Contemporary animal life held a central place in the nature lore, magical rites, and religious beliefs of prehistoric and later peoples. This fact is illustrated in such fragments of ancient art as have been preserved to us, and in the mythological explanations of nature which appear as already time-worn traditions at the beginning of the historical era. The attitude of primitive man toward the animal world—and especially the close connection between animal lore and religio-magical rites—during the thousands of years that elapsed before the birth of science could not have been without significant influence upon the earliest scientific interpretations of animal life and behavior.

The fragmentary nature of prehistoric remains makes it impossible to reconstruct with any degree of definiteness or assurance the animal lore of this long period of 'unwritten history.' Such an attempt would involve too much of the purely speculative and would carry us too far afield. In scanning the long geologic period of mankind we come upon nothing of special interest concerning the topic at hand prior to the beginning of the Upper Paleolithic epoch (c. 25,000 B.C.—Osborn). About this time the Neanderthaler race in western and southern Europe was dispossessed by the Cro-Magnon, whose culture remained dominant until nearly the opening of Neolithic times. The Cro-Magnons show a development of animal art that appears striking indeed and seems to furnish a clue to the animal lore of their age and of the stage of primitive culture which they represent. It is among this people that we come upon the earliest indications of extensive observation of animal behavior arising from what appears to be other than directly practical motives.

Upper Paleolithic Industry and Art.—The industrial deposits show clearly that the Cro-Magnon was nomadic and lived mainly by hunting and fishing. The hyena, cave-bear, and cave-lion were his mortal enemies; the bison, horse, deer and other wild animals and fishes were his daily food. A keen interest in the characteristic activities and mode of life of these animal forms would naturally be fostered by the direct, active struggle of man and animal to survive in a common environment. The superior powers and prowess of various animals forced the ancient cave man to resort to simple strategy in order to entrap and otherwise dominate them. The Paleolithic hunter and fisherman were, at their best, probably primitive naturalists, displaying deliberate and careful observation when circumstances demanded. Much of the information thus acquired doubtless entered into the verbal tradition of the times. A very considerable store of observations concerning the animal world, largely but not wholly practical in nature, must have accumulated in process of time.

The art of the Cro-Magnon, both mobile and parietal,[1] appears to have been autochthonous, and shows a gradual development from Early Aurignacian to middle or late Magdalenian times, although the various forms of it (sculpture, engraving or etching, and painting) apparently reached their height at different times during this long epoch. It is significant for our purposes that the art of the Cro-Magnon deals almost exclusively with animal form and motion, neglecting for the most part not only the plant and inanimate worlds but even man himself. Surprisingly accurate, realistic representation of contemporary animal life distinguishes the art of this period from that of even much later times, and justifies the reference of Osborn to the Cro-Magnons as the Paleolithic Greeks. The mural paintings of the caverns represent their most noteworthy attempts to portray animal form and be-

[1] Industrial or mobile art (l'art mobilier) consists of the decoration of small personal belongings, ornaments, and implements of stone, bone, and ivory; mural or parietal art (l'art parietal) consists of drawings, engravings, paintings, and bas-reliefs on the walls of caverns and grottoes (Osborn).

havior. Among the best of these are the famous polychrome frescoes of Marsoulas, Font-de-Gaume, and Altmira.

Concerning the work of the Cro-Magnon artists, Locy says:

> An immense number of these pictures are known representing the mammoth, the horse, the reindeer, the red deer, the cave-bear, the cave-lion, hyena, bison, aurochs, grouse, ptarmigan, etc. They are by no means sporadic, but are distributed over a wide geographical area, indicating that there were many individuals who observed and sketched with varying degrees of skill. Bearing in mind that the purpose of the pictures as well as the life and social condition of this remote people is conjectural, the great number of pictures suggest that there might have been a guild of picture makers (or artist-naturalists as Locy suggests in another connection) whose occupation passed on from generation to generation. The better class of pictures cannot be interpreted as the sudden inspiration of untutored savages; some of the best bear internal evidence of careful and prolonged observation. The artists were wonderful observers of limb motion and many drawings show exact proportions of head, body, limbs, horns, hoofs, etc.; others show posture, locomotion, fighting, herding, and migration.

The primary impulse behind this development of art, extending over many thousands of years, was probably neither scientific nor artistic in any modern sense, but occult; and on this assumption the pictures represent refinement in magic or religious ceremony. Some objects of mobilary art were evidently idols, fetiches, or likely were connected with totemism. Obermaier calls attention to the fact that the mural paintings and bas-reliefs are often found in relatively inaccessible recesses of caverns, and were thus apparently not meant for ordinary human observation. He believes these were used in connection with the magic of the chase, magic propitious to fertility of animals used for food, protective magic and the like. Mutilation of the representations of certain ferocious animals such as the cave-bear, tiger, etc., by weapons suggest that magic incantations were practiced upon them. The animal as well as the human enemy was evidently made weak in effigy. Whatever the source of this widespread interest, it

led to keen and accurate observation of animal behavior by primitive man thousands of years before the dawn of history.

Neolithic and Later Epochs.—Very little of special interest regarding the animal lore of the Neolithic, Bronze and Iron ages of European culture has been disclosed by the deposits so far recovered. The early stages of Neolithic culture give evidence of the domestication of the dog, horse, pig, and many other animals along with the beginnings of settled tribal life. In spite of the tremendous advance of Neolithic over Paleolithic man in most ways, his representation of animal life is on a distinctly lower level, taking the period as a whole. The drawings are usually more crude. They are marked, however, by the frequent appearance of composition, in which the human figure, usually more or less conventionalized, often appears. Hunting scenes which include the bow and arrow, or other weapon, and the now domesticated dog, are not unusual. Apparently Neolithic man was a less careful observer of animal form and behavior than his predecessor had been, or possessed less skill in the art of portrayal.

Of the later epoches, the Minoan culture, representing the Bronze age in the land of the ancient Greeks, offers the most striking examples of animal art, and suggests the existence of a rich animal lore. The famous Vaphio gold cups (see Singer) illustrate the marvellous, realistic portrayal of animal form and movement during the late Minoan period (c. 1700–1200 B.C.). The movements of the bull depicted in various postures on these cups is extremely accurate and realistic while the figuring of plants is so crude that the trees cannot be identified. "The natural interest in the animal rather than in the plant," says Singer, "might be illustrated by a hundred instances from Paleolithic cave paintings downward . . . ." Animal sculpture and polychrome mural decorations, including fresco-paintings of bull fights, also reached a high degree of perfection among the Minoans. The motive behind this development of animal art cannot be certainly known, but it is clear that interest in animal life was sufficient to bring about careful, systematic observation of behavior. Furthermore, the art itself could hardly have developed apart from a verbal tradition rich in animal lore.

Early Civilizations.—Inasmuch as the Assyro-Babylonian and Egyptian cultures were highly developed long before the close of the prehistoric era in western Europe, we might reasonably expect to find extensive animal observations of the natural history type occurring among these peoples. But such is not the case, and the reason for this lack is not far to seek. The animal lore of the times is inextricably bound up with esoteric beliefs and practises. In fact, it has been preserved to us very largely in connection with mythology, magic, and religious superstition of the times.

In these and other ancient civilizations, certain animals were held in high esteem. Animal cults were common, especially in early Egypt. The worship of the sacred bull of Apis representing the reincarnation of Ptah, the local god of Memphis, may be cited as a well-known example. Each district, or nome, in early Egyptian times had its local divinity and these were animals more often than not. The animal was either worshipped outright or was looked upon as the chthonic (earthly) embodiment or, perhaps, merely the symbol of the god. Gods possessing human form gradually displaced the cruder animal deities as general culture advanced. In many cases the god was a grotesque composite of animal and human form. Thus we find in the Egyptian pantheon the ibis-headed Thoth, the cat-headed Pacht, the ram-headed Khnum who was so often accompanied by the frog-headed goddess Heket, and many others. This same type of representation seems to have persisted in a more refined form in the angelology and demonology of the Hebrew and Christian religions of our own time. The animal lore of these early civilizations, as embodied chiefly in their mythology, doubtless influenced the early scientific conceptions of the Greeks, as it certainly did the art and literature of all later times.

The early rise of medical cults in ancient Babylonia and Egypt [2] did not stimulate scientific observations of animals of

[2] It appears that medical and surgical practice was regulated by law as early as 2250 B.C. (Code of Hammurabi) in Babylonia, and pictures of surgical operations occur on the door post of a tomb discovered by Max Muller, near Memphis, Egypt, which dates back to about the same period. The famous Ebers manuscript (c. 1550 B.C.) contains, according to Garrison, 110 pages of brief descriptions of diseases and mentions over 700 remedies.

a very high order as might have been expected. These cults were narrowly practical in spirit, and their art represented a strange congeries of incidental observation, magic, religious superstition and astrology. The demoniac theory of disease prevailed for the most part, and exorcism by magical or religious ceremony became the accepted therapy, although rude forms of surgery and various kinds of drugs were sometimes resorted to. Certain parts of animals—usually such filthy parts as blood, fat, viscera, and excreta—when variously concocted, were thought to ward off disease, or to effect cures by magical transfer of disease to another person, animal, or object. Special potency inhered in such animals as were rare, or difficult to procure. The animal part was thought of as the bearer of magical properties rather than as a natural remedy. In striking contrast to early Greek medicine, that of Egypt and Babylonia made no worthy contribution to the natural history type of animal observation.

The ancients employed animals extensively in augury, soothsaying, and similar superstitious practices connected with the prognostication of future events. A common form of augury consisted in the inspection of the state and position of the viscera (or some part of it) of the sacrificial animal. The Babylonians, according to Garrison, very early developed soothsaying by liver inspection (hepatoscopy) to a fine art. Models of the liver in terra cotta, dating back some 3000 years, have been discovered, divided into squares and studded with prophetic inscriptions. "The liver as the source of blood," says this author, "was regarded as the seat of the soul, and as the god identified himself with the sacrificial animal, to inspect the liver was to see into the soul of the animal and the mind of the god."

The importance of the animal in the artistic expression of these ancient civilizations is so well known as to require no detailed comment. In sculpture, painting, and other forms of pictorial representation, the animal held a respectable and often a dominating place, and the same is true of the art of the story teller and of literature after written language had developed. The exploits of animals, men, and gods find a

common expression in the myth and in other types of story. The genuinely scientific motive that later gave rise to natural history among the Greeks had not as yet come to birth.

Summary.—The natural interest of primitive man in the animal life about him affords the key to the understanding of early animal lore. The life of animal and man was so intimately interpenetrated that a practical knowledge of the behavior of contemporary animal forms was essential to the survival of mankind. And when the cave man attempted to portray the objects of major interest in his environment, or sought to control that environment by magical or other superstitious devices, pictorial and plastic art, in which the animal form predominated, made its appearance. For thousands of years this nomad observed the form and movement of animals and pictured the same upon his stone implements and upon the walls of his cave dwelling. The records show that he was often a careful observer of living nature and a skillful, realistic artisan. Much later, man domesticated certain of the animals—the dog for a hunting companion, and other species to serve as beasts of burden and as controllable sources of food and clothing.

In Babylonia, Egypt, and other ancient civilizations, the animal continued to play an important rôle in the cultural as well as the practical life of the people. The evolution of a mythology rich in animal lore, and of numerous animal cults, show this even more clearly than the animal art of these later times. The long contact of animal lore with astrology, magic and religion must have influenced the early development of natural history among the Greeks. For, as Singer says, the early Greek thinkers inherited 'a great mass of observations' representing the accumulations of centuries from the earlier peoples of the Euphrates, the Tigris, and the Nile, and these became the basis of their own systems. Well directed effort to develop a comparative psychology, in connection with a scientific natural history, appears first among the Greeks, as we shall see, but the heritage of the pre-scientific ages can hardly be ignored in the interpretation of this later movement.

II. The Greek Influence

Early Greek Thinkers.—The birth of the scientific movement among the Greek philosophers of the sixth century B.C. very naturally brought biological problems of major importance to the fore. For the most part these pioneer thinkers rejected, at least in theory, the mythology, magic, and other superstitions inherited from earlier times, and attempted to find in nature herself the key to the riddle of the universe. These early thinkers are mainly important as the forerunners of Aristotle who, as the founder of natural history, zoölogy, and comparative psychology, may be taken as the embodiment of the Greek influence in these closely allied fields.

The early Greek philosophers were much concerned with the problem of the origin of life, and many of them, including even Aristotle himself, believed in the spontaneous generation of life from inorganic matter. Their speculations concerning the origin of the great variety of plant and animal forms seem to foreshadow in certain cases, as Osborn has so interestingly shown, modern conceptions of evolution. Another biological problem that led to much discussion was that of attempting to account for the apparent universality of the phenomenon of the adaptation of living forms to their natural environment, and of parts, or organs to the functions which they normally perform. In general they were teleologists, and here again Aristotle should be included; they appealed to the principle of design, or purpose—either external or internal—to account for this, as for all other apparent harmony within the universe. Empedocles, however, dared to set forth the doctrine that nature produced myriad forms by chance and only those survived that happened to fit their environment—a notion somewhat akin to the doctrine of natural selection—and for this denial of cosmic purpose Aristotle takes him severely to task. These and other like questions, raised by these ancients, are still problems of deep concern to the modern biologist, and indirectly bear upon comparative psychology. These early philosophers were not naturalists in our modern sense. Their interest was mainly speculative and they did

not make observations of animal activities of the more extensive, systematic and exact sort which later characterized the work of Aristotle.

The development of Greek art and of Greek medicine previous to the time of Aristotle aids us in understanding his own marvellous accomplishments in zoölogy. Greek art from about the seventh century downward shows, according to Singer, 'a closeness of observation of animal forms that tells of a people awake to the study of nature.' In illustrating the realistic animal art of the times, he cites the following representations of animal behavior among others: "We see antelopes quietly feeding or startled at a sound, birds flying or picking worms from the ground, fallow deer forcing their way through thickets, browsing peacefully, or galloping away, boars facing the hounds and dogs chasing hares, wild cattle forming their defensive circle, hawks seizing their prey. Many of these exhibit accurate observation."

Here, as in the Paleolithic art, plants seldom appear, whereas such details as dentition, the different muscles, and the color and direction of hair, are often accurately portrayed in the animal figures. It does not seem so strange, in view of this widespread art, that Aristotle, several centuries later, should have illustrated his animal studies with anatomical diagrams.

The practice of dissection among Greek physicians extends as far back as the sixth century. Singer is authority for the statement that there appears in certain writings of the famous Hippocratic Collection, probably antedating Aristotle by two centuries, an unmistakable attempt toward a classification of animals (Coan classificatory system). Observations on the nature and habits of animals, on inheritance and other related topics, are also included. Later pre-Aristotelian medical writings often involved material of general biological interest. In this connection we should remember that the father of Aristotle was himself physician to King Philip of Macedonia, and a member of the Asclepiadæ.

Aristotle, the Naturalist.—Aristotle (384–322 B.C.) has been generally distinguished as the first systematic writer on

zoölogy and as the father of natural history. He deserves with equal justice to be considered the founder of comparative psychology. He wrote three sizable treatises on animals,[3] and two tracts of lesser importance, and in addition had much to say about animals in his other scientific and philosophical writings. To the five works mentioned should also be added his 'De Anima,' which treats of animal as well as human mental life, and is, in fact, much more of a comparative than a strictly human psychology in our modern sense. Inasmuch as Aristotle wrote from the larger viewpoint of natural history, his contribution to comparative psychology must be gleaned from various sources, although the major topics are systematically treated in his 'History of Animals.' It is worthy of note that Aristotle does not consider his zoölogical description of an organism or species complete until he has covered its psychological as well as its anatomical, physiological and developmental aspects.

Aristotle approached the study of the animal world in the true spirit of the naturalist and with the methods of the best science of his day, to which he himself was no mean contributor. According to Hertwig he may be considered as having founded zoölogy as a universal science, 'since anatomy and embryology, physiology and classification find equal consideration.' His classification of animals [4] was scarcely improved upon before the time of Linnæus. Some of his findings in morphology, evidently based upon careful dissection, remained unverified by later observers until very recent times. His anatomical diagrams, to which he often makes reference, have unfortunately not been preserved to us.

The scope of his interest as a naturalist is indicated by the

[3] The three larger treatises are: *Historia animalium*, or Natural history of animals, *De partibus animalium*, or concerning the parts of animals, *De generatione animalium*, or concerning the generation of animals. The two tractates deal with the *motion* and *locomotion* of animals.

[4] Aristotle employed two categories in classification, the γένος, or group, and the εἶδος, or kind or species. Of the former there were eight as follows: mammals, birds, oviparous quadrupeds, fishes, mollusca, crustacea, insects, and animals with shells. The first four groups, corresponding roughly to the vertebrates, he termed ἔναιμα (blooded, or with red blood) as contrasted with the αναιμα (bloodless, or with colorless blood) which included the four remaining groups.

fact that he made reference to over 500 species of animals in his 'History,' and these cover an exceedingly wide range, including many exotic forms. His treatment of a given species or form comprised such topics as breeding and nesting behavior, care of the young, hibernation, methods of locomotion, feeding, fighting, migration, etc. It is evident from his detailed descriptions that he had spent many hours carefully observing bees and other insects, many species of birds and mammals, and the fishes and other marine forms that swarmed the shores of his native land.

However, we must not suppose that these writings of Aristotle are wholly, or even largely, a chronicle of his own original observations, or represent entirely his own genius. They include much that is admittedly drawn from earlier and contemporary sources duly sifted and systematized by the author. His students of the Lyceum were probably responsible for certain parts of the material—notably such as do not measure up to the usual high standard of the master, either in content or form. In addition to being a pioneer investigator of supremely high merit, Aristotle had the genius to preserve for us a systematic account of the existing state of knowledge concerning the animal world. Darwin was thinking of Aristotle in the rôle of investigator when he wrote to Huxley: "Linnæus and Cuvier have been my two gods though in very different ways, but they were mere schoolboys to old Aristotle."

Aristotle's Viewpoint and Method.—Aristotle, as is well known, was both a vitalist and a thoroughgoing teleologist. He ascribes all the characteristic phenomena of living forms, both plant and animal, to the operation of an imminent vital principle or soul (ψυχή). In his conception, the individual organism like the universe at large became the natural expression of an all-pervading design or purpose; it must be thought of as the concrete realization, both in structure and function, of an inner perfecting principle, the entelechy. "Nature," as Aristotle puts it, "does nothing without aim." The soul, in the system of Aristotle, becomes the formal, the efficient, and the final cause; it determines the organization,

the movement—whether inner or outer—and becomes the ultimate purpose or end of the organism. For the animal, at any rate, the soul does not exist as a separate entity apart from the body; it comes into being when formless, inert matter (the material cause) begins to take form and exhibit living functions; it is responsible for the structure-function characteristics of the organism at every stage of the life cycle.

This vital principle, or soul, at different levels of manifestation, was held, by Aristotle, to explain the whole range of functioning from the nutritive processes (assimilation, growth, decay) to the psychic activities of imagination and reason. The nutritive faculty only is found in plants; the sensory or perceptual faculty in addition characterizes even the lowest animals; most, if not all, animals possess also a third faculty involving appetites, or desires coupled with the power of locomotion as a means of satisfying such appetites. The fourth, or rational faculty is possessed in the strict sense only by man himself. The soul thus becomes, in the system of Aristotle, the all-important and ultimate principle by means of which the organism lives and moves and, under certain limiting circumstances, also reasons.

Animals are motivated by sensations of pleasure and pain, appetites, desires, and conscious purposes; self-preservation and reproduction being the natural and proper ends about which the life-activities of the organism concentrate. It occasions no surprise, therefore, to find the behavior of even the lowliest organisms described by Aristotle in the quaint terminology of conscious purpose. He ranks the sponge as little higher than the plant world, and yet it contracts—so he thinks—when it 'becomes aware of an attempt being made to pluck it' from the rocks; in boisterous and windy weather it contracts, 'obviously with the object of tightening its hold.' Distinctions of pleasantness and unpleasantness he finds to be coextensive with sensation, and this fact is of great importance inasmuch as 'all animals pursue pleasure in keeping with their nature,' and likewise avoid the painful.

Aristotle's work on animals is distinguished by the constant emphasis placed on the inductive method, in spite of its

intimate connection with his general philosophical system. The conclusion to his discussion of method in the introductory chapter of his 'History of Animals' might easily be taken for a statement from Bacon or a modern scientist. "Experience," he says, "furnishes the particular facts from which induction is the pathway to general laws." He insists, moreover, that these general laws must always be sought in, and agree with, observed facts. His own conclusions are often based wholly or in part upon the reports and conclusions of others, but only when such material appeared to be based upon actual observation. The immense scope of his work would naturally preclude original observation in most cases. That he is somewhat credulous in respect to hearsay and anecdote cannot be denied. Nevertheless, he is often critical of such evidence, and usually careful to specify its source so that the reader may form his own judgment.

The consistent use of the comparative method by Aristotle in dealing with the morphological, physiological or psychological aspects of the organism is very striking and offers a source of constant surprise and delight to the modern reader. Since he includes man among the animals—indeed, makes him the basic type structurally and functionally—his method is comparative in the highest and broadest sense. Man is selected as the type evidently for pedagogical reasons, for 'man is the animal with which we are all of us the most familiar,' and one must needs start from the known. The comparative method in psychology is thus as old as the science itself, since both find their beginning in Aristotle.

The Senses of Animals.—There are only five senses, according to Aristotle, in either animal or man, namely: touch, taste, smell, hearing and vision. He assumes the presence of touch in all organisms and attempts to determine the presence or absence of the other four senses in each of his main groups of animals. His first appeal is to inference from structure; if a sense organ is present, roughly analogous to that of man, no further evidence is required. A wolf, for example, must possess smell, hearing, and vision, since it has the appropriate organs, nose, ears, and eyes. The question of essential and

accessory structure apparently did not enter. In the absence of a given sense organ, or when such could not be certainly identified, recourse was had to behavioristic evidence The response of the organism to characteristic stimuli, or the report of such response by other observers, was taken as evidence. In any case observations were rough and uncontrolled and almost wholly lacking in scientific value. Much of Aristotle's evidence in this field appears to be drawn from the folklore of the times.

The following illustrations are fair examples of his data and method. The scallops probably possess vision, since they close their valves when the finger is moved toward them 'as if they could see what you are doing.' The selection of certain flowers, and especially the fresh sweet ones, by the bee in foraging affords convincing evidence that insects possess the sense of taste. The octopus and the crayfish distinguish smells, since they can be caught by bait, or dislodged from rocks by fleabane. Likewise the power of smell is proven in the eel by the fact that an earthen pot that has previously contained pickles makes an excellent eel trap. That fishes possess smell is shown by the fact that different baits are used more effectively for different kinds of fishes, and by the hearsay reports that aggregations of fish swim away from the foul fish-washings that are cast overboard by fishermen.

In view of the present controversy over the question of hearing in fishes, the observations of Aristotle on this point are interesting. He cites, as positive evidence, the fact that the bull-head, a river fish which burrows under the rocks, can readily be frightened out of its hiding place by dropping stones in the water above, which make a clattering noise on striking its rocky hiding place. Shoals of dolphin, he avers, are 'stupefied with the noise' and easily caught when a loud splashing in the water with oars is made by fishermen. He cites a few similar cases, and then naïvely concludes: "From these facts it is quite obvious that fish can hear; and indeed some people from living near the sea and frequently witnessing such phenomena, affirm that of all living creatures the fish is the quickest of hearing."

His final conclusion concerning the distribution of the senses in animals is that mammals, birds, oviparous quadrupeds, and fishes possess all five senses. Of the other four groups, corresponding roughly to our invertebrates, he finds that the mollusca (cephalopods), crustacea, and insects have all the senses except possibly hearing; while the lowest group (sponges, cœlenterates, etc.) possess only touch, taste and smell.

Having settled to his own satisfaction the question as to the distribution of the senses, Aristotle apparently feels that his task is done. He does not mention the problem of range and intensity differences in the various senses in different species, other than to make a general statement occasionally concerning the special development of one or another sense in a given species. Nor does he consider the matter of possible differences in the subjective quality of the sense impression from different types of sense-organ representing the same sense. To Aristotle an ear was an ear and an eye an eye 'and nothing more.' While holding in general that man is the perfect animal, he freely admits that some one sense in certain animals is much superior to that same sense in man.

Memory and Intelligence of Animals.—Concerning the higher intelligence of animals Aristotle makes the statement that 'many animals have memory and are capable of instruction; but no other animal except man can recall the past at will.' This difference is apparently the most fundamental one between the animal and the human intellect, as he sees it. The higher animals may, and many of them do, possess a passive imagination involving the revival, under appropriate conditions, of residual sense-impressions. But they do not possess the active or creative imagination based upon voluntary recall which is so characteristic of the higher rational life of man.

Animals have a certain practical intelligence; a low order of inference may be exhibited on occasion. For, 'just as we find in man knowledge, wisdom, and sagacity, so in certain animals there exists some other potentiality akin to these.' And while 'of all animals man alone is capable of deliberation,'

some animals possess 'something akin to sagacity.' As evidence of inner representation of some sort he appeals to the apparent dream life of animals. Practically all mammals dream, in his opinion, and dogs 'show their dreaming by barking in their sleep.' In general, however, Aristotle is of the opinion that the animal is pretty much the slave of present stimuli, the memory image being invoked even in the higher types only on occasion. Although he finds that 'in the great majority of animals there are traces of psychical qualities or attitudes,' this need not mean more than that they are sensitive to sense impressions.

In thus marking off man from the other animals as possessed of creative imagination and the true rational faculty, Aristotle is evidently speaking of the normal adult mind. For he states in this connection that 'in children may be observed the traces and seeds of what will one day be settled psychological habits, though psychologically a child hardly differs for the time being from an animal.' The rational element in the mental makeup thus becomes the important criterion of the higher and human, as contrasted with the lower and animal—a distinction which has prevailed for the most part from that time forward in the field of psychology.

Little evidence is brought forward to illustrate higher intelligence in the animal world. Such cases as are cited are chosen without distinction from both the so-called instinctive and learned types of response. This is significant as indicating that Aristotle made no clear-cut distinction between instinct and intelligence. And it is doubtful whether such a dichotomy was made until many centuries later, although Galen (c. 130 A.D.) and certain of the later Roman writers appear to make the distinction fairly definite.

The evidence for animal intelligence which Aristotle cites is mostly commonplace observation and anecdote. He speaks of a certain species of spider as 'preëminently clever and artistic' on account of the fact that it is able to build a web of unusually intricate design. A domesticated woodpecker had been seen to insert an almond into a hole in a piece of timber 'so that it might remain steady under its pecking'

according to the report. In the vicinity of Lake Maeotis, so he had heard, 'wolves act in concert with fishermen, and if the fishermen decline to share (the catch) with them, they tear their nets in pieces as they lie drying on the shore of the lake.' But it is the elephant over which Aristotle waxes the most enthusiastic. It is 'of all wild animals the most easily tamed and the gentlest.' Furthermore, 'it can be taught a number of tricks, the drift and meaning of which it understands; as for instance, it can be taught to kneel in the presence of the king. It is very sensitive, and possessed of an intelligence superior to that of other animals.' Man, then, is the most intelligent of animals, and the elephant next; the relative position of the remaining types in the scale of intelligence was apparently left for later generations to determine.

Temperament and Emotional Life.—The observations of Aristotle relative to the temperamental and emotional life of animals are of the most general and desultory sort. He does little more than enumerate the human passions which he ascribes to the higher animals without hesitation. His contribution at this point is mainly that of recording commonplace opinions drawn from the animal lore of his day. The following quotation from his 'History of Animals' is a fair sample of his manner of treating this topic.

> Animals also differ from one another in regard to character in the following respects: Some are good tempered, sluggish, and little prone to ferocity, as the ox; others are quick tempered, ferocious, and unteachable as the wild boar; some are intelligent and timid, as the stag and the hare; others are mean and treacherous, as the snake; others are noble and high-bred, as the lion; others are thoroughbred and wild and treacherous, as the wolf; . . . Further some are crafty and mischievous as the fox; some are spirited and affectionate and fawning, as the dog; others are easy tempered and easily domesticated, as the elephant; others are cautious and watchful as the goose; others are jealous and self-conceited, as the peacock.

Concerning sex differences in temperament he writes:

> In all cases, except those of the bear and leopard, the female is less spirited than the male. . . . With all other animals the female

is softer in disposition than the male, is more mischievous, less simple, more impulsive, and more attentive to the nurture of the young.

The male is described as being more savage and less cunning. The sex difference is greater, in his opinion, among the higher types, and especially so in the human species. Woman is more compassionate than man, he declares, but is also more jealous and querulous, more given to despondency and pessimism, 'more void of shame or self-respect, more false of speech, more deceptive' and more shrinking and cowardly. That the male is more courageous than the female he illustrates by the alleged fact that the male cuttlefish will aid the female in avoiding capture, while the female will scurry away when the male is attacked, leaving him to fight alone.

It will be noted that many of the terms employed by Aristotle to describe the animal temperament are those used in common speech to designate character traits in human beings, and have a more or less specific ethical connotation. And it can hardly be doubted that Aristotle meant that these terms should carry the usual ethical implications in his characterization of animals. Craft and treachery, jealousy and self-conceit, nobility and courage were applicable to man and animal alike. Sex behavior is treated in much the same vein. He speaks of the mare as being the 'most sexually wanton' of animals; of the partridge as 'salacious' because of its frequent copulation, while the crow is 'inclined to chastity' for the opposite reason. These statements are much more than merely anthropomorphic figures of speech; they are, in fact, the logical outcome of the all-embracing teleology that was fundamental in Aristotle's systematic interpretation of the Universe.

Later Greek and Roman Writers.—Interest in the animal division of natural history gradually declined after Aristotle. The later Greeks following the natural bent of the Hellenic mind engaged themselves in philosophical, and particularly ethical speculation. The Romans in general borrowed their natural science from the Greeks, and produced no investigator of consequence in this field. Plutarch and Galen from among

the Greeks, and two Roman writers, Seneca and Pliny, may be taken as representative of the half-millenium following Aristotle. These will be treated briefly in chronological order.

Seneca (54 B.C.–39 A.D.)gives a good account of the view-point of the Stoic concerning the intelligence of animals. He denies to the animal the power of reason; man alone is rational and capable of ethical conduct. Any similarity between the mental faculties of man and animal is only apparent. The lion does not feel genuine anger when it stands ready to tear the passerby in pieces; the animal who may save a man's life is not possessed of a feeling of generosity, or of desire to be of service. Animals are unable to recall past experience, their memory capacity being limited to recognition. Thus the horse may recognize a road over which it has traveled before but remembers nothing of it when in the stable afterward. Seneca seems to make a clear distinction between native and acquired behavior. The former is marked by uniformity, 'takes place without reflection,' and involves only such know-ledge as is necessary for self-preservation. As the Stoic ex-alted the rational faculty, so also he exalted man, the rational animal, and set him apart in sharp contrast to the brute creation.

Pliny the Elder (23 B.C.–79 A.D.), a Roman general, was a compiler and popular writer in the field of natural history. His 'Natural History' is an encyclopedic collection of 'anec-dotes, sayings, traditions, and citations from earlier writers' and from contemporary sources, covering a wide range of scientific topics (20,000 as he claimed). He held the mental powers of animals in great esteem and in the section devoted to animals (books 8–11) includes many anecdotes to support his opinion. He is especially impressed by the honesty, discretion, sense of justice, docility and memory capacity of the elephant. He cites its public performances in the Roman amphitheatre—dancing in concert, wielding arms, walking ropes, and similar stunts. He tells of one elephant that was found practicing his dancing lesson alone at night in the moon-light after being punished severely for doing poorly that day; of another who could write the following sentence in Greek,

'Ipse ego haec scripsi, et spolia Celtivi dicovi.' Although Pliny's work was thoroughly uncritical in spirit, it was widely read in his own day and for centuries later.

Plutarch (c. 46–120 A.D.), the noted biographer, discussed at some length his own and current views concerning animal intelligence. He opposed the view of the Stoics that animals have no real thought or feeling life. He brings forward, after the manner of Pliny, much anecdotal evidence to prove the sagacity and human-like emotional capacity of the higher animals.

Galen (c. 130–200 A.D.) has been called the founder of experimental physiology and experimental medicine, and his voluminous writings on anatomy were the accepted authority until the time of Vesalius (sixteenth century). He dissected a large number of mammalian types including an elephant and many apes. He was the last and perhaps the greatest biological investigator of the Alexandrian school of Greek science. Although he was primarily interested in anatomy and in function in the narrow, physiological sense, there are scattered throughout his works many original observations of animal behavior. An interesting example is that of a kid found to be alive after dissection from the mother and 'snatched away before it saw its dam.' The kid, he observes, first got upon its feet and walked, 'then it shook itself, and afterwards scratched its sides with one of its feet.' Upon being released at once in a room where Galen had placed vessels containing wine, milk, various other liquors, grains, fruits, etc., it smelled of them all and finally drank the milk. Galen insistently calls attention to the apparent adaptation of structure to function which he sees everywhere, and seeks to explain this by the principle of design. As Holmes has pointed out, he shows an approach to the concept of instinct as this term came to be used by later writers.

Summary.—The contribution of the Greeks to comparative psychology as well as to most other aspects of zoölogical science may be summed up in the person and works of Aristotle. He preserved to us a systematic account of the existing state of knowledge based upon his own wide observa-

tions and the winnowings of the animal lore of earlier times. His conception of the animal and of its behavior was vitalistic and teleological and his interpretations naïvely anthropomorphic—due to the fact that his biological observation and his philosophical speculation were intimately related. His work is marred by a rather extensive use of the anecdotal method. He emphasized the importance of the inductive method both in theory and practice, and developed psychology as well as general biology along broadly comparative lines. He was a careful, patient investigator and probably made a greater contribution to comparative psychology than any other writer previous to Darwin.

The decline in the natural science type of animal study was rather sharp. Later Greek and Roman writers turned to metaphysical and ethical speculation, or became mere compilers along scientific lines. The Stoic stressed the distinction of Aristotle between man, the rational animal, and the remaining animal world much more than he himself had done. By the time of Galen, who stands out as the only important Greek investigator after Aristotle, a fairly definite concept of instinct appears to have emerged. The foundation had thus been laid for a sharp dichotomy between man and the brute creation—the one possessing reason and guided by experience, the other endowed with an instinctive life in which individual experience plays at most a subordinate rôle. This distinction, was further developed as we shall see, in connection with the doctrines of the church and became the ruling conception in the discussions of the mental life of animals during the middle ages.

III. The Early and Middle Ages

Early Influences.—The rise and spread of Christianity, among other things, hastened the decline of natural science to the low level characteristic of the early and middle ages. The world-renouncing attitude fostered by early Christian teaching later developed into one of contempt for natural phenomena and of open hostility to the observation of nature. Man and his redemption became the one problem of supreme importance, and the animal was degraded and despised in

common with the physical world. A sharp dichotomy was drawn, on theological grounds, between mankind and the beasts that perish, by placing undue stress upon the superiority of the one and upon the inferiority of the other. Reason was denied the animal in strictly orthodox circles, inasmuch as the possession of reason might also imply moral responsibility and the possibility of redemption and immortality. Aristotle had assigned to man, as the most perfect animal, the highest place in the economy of nature; Christian theology placed him outside and above nature, insofar as his most fundamental relationships were concerned.

The influence of this theological conception was strengthened by the fact that education gradually passed into the hands of the ecclesiastical authorities where it remained for centuries. Greek science, except for the medical system of Galen, was looked upon with the same suspicion that attached to pagan culture in general. The natural science of Aristotle, translated by the Arab scholars, first in the eighth, and later in the tenth and eleventh centuries, exerted little influence on Christendom until the time of Albertus Magnus. Aside from the Bible, Pliny and the 'Physiologus' were the most popular sources of information on animals during the millenium immediately following Galen.

The 'Physiologus' was a popular work on natural history, dating back at least to the third century (A.D), dealing with stones, trees and animals, and appears to have been widely circulated in one form or another even as late as the twelfth century. It came in time to include only animals and was then called the Beastiarius. It treated about 70 animals, many of which were such mythical forms as the dragon, basilisk, unicorn and phoenix. It was issued in both prose and poetical form and was often beautifully illustrated. Very early the natural history element in it became subordinated to the allegorical; the animal stories were made to symbolize moral and religious beliefs. The following from Locy (who quotes from White one of the stories on the lion) will serve as an illustration: "The lioness giveth birth to cubs which remain three days without life. Then cometh the lion,

breatheth upon them, and bringeth them to life. . . . Thus it is that Jesus Christ during three days was deprived of life, but God the Father raised Him gloriously." Authorities differ widely in their opinion as to the magnitude of the influence of the 'Physiologus' during the middle ages. It appears to have been quoted by Albertus Magnus and contemporary encyclopedists of the thirteenth century.

The Revival of Aristotle.—The latter part of the middle ages, and notably the thirteenth century, was marked by the revival of the writings of Aristotle on natural history and other subjects. There also appeared the beginnings of independent observation which, however, in things biological made little headway previous to the opening of the sixteenth century. The writings of Aristotle, as of the Greek philosophers generally, had been preserved by the Saracens during the preceding centuries. Avicenna (980–1037) had written a paraphrase of parts of Aristotle's works on natural history and Averroes (1126–1198) had issued a commentary on the 'History of Animals,' not to speak of earlier Arabic translations. But stubborn opposition to 'infidel,' as well as to earlier pagan culture, prevented this revival of Aristotle among the Moslems from exerting any great influence upon Christendom, although the fact must not be overlooked that many Christian students attended such famous Mohammedan universities as that at Cordova. It was necessary that the works of Aristotle be made acceptable to the ecclesiastical authorities and be widely distributed if the new learning was to prevail.

Before we speak of the work of Albertus Magnus and the encyclopedists who were to accomplish these important ends, brief mention should perhaps be made of Adelard of Bath, a traveler and scholar of the twelfth century. In his well known treatise 'Quaestiones Naturales' he sets forth, in the form of a dialogue with his nephew, the ideas gathered from years of searching for wisdom in many lands, and particularly those of Arabian scholars. "Adelard upholds scientific argument and investigation against a narrow religious attitude," and places reason above authority. At one point in the discussion the nephew asks: "Would you now discuss the ques-

tion as to whether brutes have minds?" Adelard replied, "On this point there is today a difference of opinion. The common people unhesitatingly say no, while philosophers are equally confident about the opposite." He then proceeds to offer reasons for this latter view, as held by the Arabs, which sound strikingly Aristotelian, and probably represent the influence of Aristotle on Arabian culture. Adelard was a contemporary of Averroes, as well as of Abelard of scholastic fame.

Albertus Magnus (1206–1280) may be considered the outstanding figure of the thirteenth century in the field of natural history and, therefore, of the middle ages taken as a whole. For it was mainly through his efforts that Aristotle's natural history was made acceptable to the Church authorities; and we must not forget that even as late as 1210 the science of Aristotle was proscribed at the University of Paris. Furthermore, Albert stoutly upheld 'the rights of observation, experience and induction' (de Wolf), and was undoubtedly himself a naturalist of considerable ability. His 'De Animalibus' is a paraphrase of Aristotle's 'History of Animals,' with comments of his own, and with an addition of 7 books (the whole comprises 26 books) which represent material not found in Aristotle, drawn largely from other previous writers and from his own experience. Albert has no scruples against rejecting the opinions of the ancients when these do not coincide with the observations of himself or of his contemporaries. He was critical enough to reject much of Pliny, and to make a clear distinction between actual observation and hearsay information, when evaluating the materials of other writers in this field. His general point of view is essentially that of Aristotle except for the fact that he makes a sharper dichotomy between man and animal in accordance with his theological prejudices. Animals, he thinks, are 'directed by their natural instinct and therefore cannot act freely'; the behavior of the insect is not accompanied by intellectual activity, but results from 'natural dispositions' as shown by the fact that 'they act always in the same way.' Their behavior is nevertheless purposive even though not deliberative,

and at its best involves a sort of practical judgment (*vis æstimativa naturalis*). In brief, Albert is Aristotle with a theological bias.

The revival of Aristotle was also furthered by the contemporary encyclopedists who included as a rule large sections on animals compiled from every available source. The largest of these encyclopedias, the 'Speculum Majus' of Vincent of Beauvais, a Dominican, includes six books devoted to animals and plants (Locy). That of the Franciscan, Bartholomew Anglicus, 'De proprietatibus rerum,' enjoyed a wide circulation, being translated into most of the vulgar tongues of Europe before the opening of the 16th century. As Locy says, this latter was the book from which Shakespeare and other Elizabethan writers drew much of their knowledge of nature. Thomas of Cantimpré wrote 'De Naturis rerum,' which was translated about the latter part of the fourteenth century by Conrad von Megenberg as 'Das Buch der Natur,' which, in this form, was widely circulated in Germany. All three encyclopedias appeared about the middle of the thirteenth century, and drew heavily upon Aristotle for their natural history material. They made no claim to originality and the compilations show a considerable lack of critical judgment. Nevertheless, they were important agencies in the dissemination of what was then known about the animal world, and in the revival of the Aristotelian tradition. The spirit of Aristotle, with its emphasis upon empirical fact and method, however, did not fully awake until after the scientific revival of the sixteenth century.

Other Influences.—The thirteenth century was the age of Roger Bacon as well as of Albert, and both of these leaders were strong advocates of the use of the inductive method in natural science. Pouchet has rated Albert as 'the greatest genius of his time,' although this rank is usually accredited to Bacon. Both were very far from being modern experimentalists, as Thorndike has shown; both were, in fact, often victims of childish credulity, and believers in the black art in one form or another. Thus we find Albert believing in the secret power of gems and reporting that when a ring was once

drawn around a toad and an emerald, the latter 'began to crack like a nut' and broke apart when the toad gazed at it unswervingly for a short interval, after which the toad moved away 'as if it had just been freed from the power of the stone.' Albert had expected the toad to burst instead, provided the gem was 'in full vigor,' which apparently was not the case. He is also much impressed with the value of various parts of a lion to cure madness, jaundice, cancer, and to keep away moths, etc. Bacon also believed in the magical power of gems, plants, and certain animal parts, and attributed this power to the influence of the stars. He tells of a snake cut into small sections, except that the skin of the belly was left intact, which crawled to a certain plant and was 'instantly made whole' by touching it. Superstitions of this sort were widespread among the populace and hindered the development of a genuinely scientific natural history.

The attitude of the Church on animal intelligence during the later middle ages is best represented, perhaps, by the famous pupil of Albert, Thomas Aquinas (1225–1274). These two Dominicans were both devoted to Aristotle and hold much the same general viewpoint regarding the nature of the animal mind. Both make a sharper distinction than their master had done between the rational soul of man and the sensitive soul of the animal, although Aquinas admits that the human embryo possesses at first only the latter, 'a more perfect one which is both sensitive and rational' being provided by act of deity at some point before maturity. Animals 'do not work like intellectual agents, in diverse and opposite ways, but just as nature moves them to fixed and uniform specific activities.' Here, as with Albert, the behavior of the animal is essentially instinctive, while that of man is rational and involves freedom of choice. Instinct and reason, man and beast were thus set in contrast; the distinction that Aristotle the naturalist had been unable to make in this strict sense, was made by the theologians, and largely on religious rather than scientific grounds.

Summary.—This period was one of dearth so far as the investigation of the mental life of animals is concerned.

Natural history study sunk almost to the low level characteristic of the ancient civilizations of the pre-scientific age. The animal was either ignored along with the world of nature in general, or became the subject of superstitious beliefs. In the early part of the period the ignorance of the masses and the hostility of theology to natural science observation were largely responsible for the retrograde condition. In Adelard of Bath, a pioneer thinker of the twelfth century, we note the beginnings of a reaction against the prevailing view. The following century witnessed the revival of Aristotle's works on animals in Christendom, largely through the influence of Albert. The contemporary encyclopedists also aided in stirring up an interest in the animal world, inasmuch as their writings, compiled from Aristotle and other authors, were widely circulated. The orthodox view of the later middle ages made instinct and reason antithetical, and established a sharp dichotomy between man and the beasts that perish. The revival of Aristotle, however, marked a great forward step, since the 'Physiologus' and Pliny had been the earlier favorites. Once the writings of Aristotle were accepted, they became a new source of authority rather than a stimulus to further observation of animal activities. This in part accounts for the fact that there was little genuine progress before the revival of science in the sixteenth century.

(To be concluded)

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